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THEORIES OF MANURE AND FERTILIZER ACTION[1]

IT is to Liebig that we owe the first general theory of the nutrition of the plant and the function of fertilizers: although Liebig himself did not add anything to the knowledge of the process of carbon assimilation which had been acquired by Priestley, Senebier and others, nor to the study of the nitrogen and ash constituents which had been begun by de Saussure, he yet drew up from these facts a coherent theory of the course of nutrition and put it before the world with such vividness that it forthwith took its place in the general body of accepted scientific opinion. Liebig argued that since the ash constituents alone are drawn from the soil, it is only necessary, in order to ensure the complete nutrition of the plant, that there shall be no deficiency in the inorganic materials which are left behind when the plant is burnt. According to Liebig the function of the fertilizer is to supply to the soil the materials removed therefrom by the crop, and the fertilizer required can be ascertained beforehand by the analysis of a similar crop, so that the soil can be supplied with the exact amounts of potash, soda, magnesia, lime, phosphoric acid, etc., which would be removed by a normal yield of that particular crop. Neglecting Liebig's misconception of the source of the plant's nitrogen and the long controversy which arose as to the necessity of its artificial supply, we can restate the theory as assuming that

[1] A lecture given at the Graduate School of Agriculture, Cornell University, July, 1908.

the proper fertilizer for any particular crop must contain the amounts of nitrogen, phosphoric acid, potash and other constituents which are withdrawn from the soil by a typical good yield of the plant in question.

In this form the opinion, that the composition of the crop affords the necessary guide to its manuring, prevailed for some time and still survives in horticultural publications, but the course of field experiments, particularly those at Rothamsted, and the accumulation of farming experience soon demonstrated that it was a very imperfect approximation to the truth. Liebig's theory fails because it takes no account of the soil and of the enormous accumulation of plant food therein contained. Water-culture experiments demonstrated that certain elements, *e. g.*, sodium and silica, though universally present in the plant's ash, are unessential to its nutrition; field experiments also showed that other elements—magnesium, calcium, chlorine, sulphur, iron—though essential, are always supplied in sufficient quantities by all normal soils. Thus the elements to be supplied by the fertilizer became reduced to three—nitrogen, phosphorus and potassium—and even the amounts required of each of these are not indicated by the composition of the crop. To take an example—normal crops of barley and wheat would withdraw from the soil approximately the following fertilizing materials:

	Yield of Grain, Bushels	Pound per Acre Removed		
		Nitrogen	Phosphoric Acid	Potash
Wheat.....	36	50	21	29
Barley.....	48	49	21	36

Now the results of field experiments, which are abundantly confirmed by ordinary farming experience, go to show that the yield of wheat is chiefly determined by the supply of nitrogen; phosphoric acid is of secondary importance and only on exceptional soils will there be any return for the application of potash.

With barley, though its composition is very similar to that of wheat, the results are very different; nitrogen is still the most important element in nutrition, but phosphoric acid has equally marked effects, whilst in ordinary soils potash counts for little or nothing.

This may be illustrated from the Rothamsted experiments, and the part played by the reserves in the soil will be made evident by comparing the results obtained in the first and the fifth series of ten years.

AVERAGE YIELD OF BARLEY GRAIN, HOOS FIELD, ROTHAMSTED

Plot	Manuring	Average Yield of Grain, Bushels	
		First Ten Years, 1852–1861	Fifth Ten Years, 1892–1901
4 A	Complete fertilizer—Nitrogen, phosphoric acid, potash	46.1	36.3
3 A	Phosphoric acid omitted—Nitrogen and potash	35.0	22.1
2 A	Potash omitted—Nitrogen and phosphoric acid	45.6	28.0
1 A	Nitrogen only	33.6	16.6
4 O	Nitrogen omitted—Phosphoric acid and potash	30.5	12.8
1 O	Unmanured	22.4	10.0

The analysis of the barley plant would indicate that it requires nitrogen in the largest amounts, then potash and least of all phosphoric acid, but if the results for the first ten years of the experiment are considered it will be seen that the omission of either nitrogen or phosphoric acid from the fertilizer causes a big decline in yield in comparison with that of the completely fertilized plot. The omission of potash, however, is of little or no moment, since it only causes the yield to fall from 46.1 to 45.6 bushels per acre. Evidently the soil was able to supply all the requirements of the plant for potash despite the large

amounts which the crop removes. In the latter years of the experiment this stock of available potash in the soil had become somewhat depleted, so that the omission of potash from the fertilizer reduced the yield from 36.3 to 28.0 bushels per acre. The exhausted soil in these latter years causes the crop to respond to the constituents of the fertilizer only when they are all present together; taken singly, they increase the yield but little and the omission of any one of them reduces the crop almost to the minimum produced on the unmanured crop. The soil has thus become but a small factor in the nutrition of the crop, whereas as regards potash it was a very large one at the beginning of the experiment, and the defect of Liebig's theory was to neglect it entirely.

These differences in the manurial requirements of wheat and barley, differences which would not be apprehended from their respective compositions, may be correlated with the habits of growth of the two plants: wheat is sown in the autumn after but a slight preparation of the ground, nitrification is thus restricted, especially as the chief development of the plant takes place in the winter and early spring before the soil has warmed up, and as a consequence the crop is particularly responsive to an external supply of some active form of nitrogen. On the other hand, the wheat plant possesses a very extensive root system and a long period of growth, hence it is specially well fitted to obtain whatever mineral constituents may be available in the soil. In ordinary farming the only fertilizer used for the wheat crop will be a spring top-dressing of 100 pounds per acre or so of nitrate of soda or an equivalent amount of sulphate of ammonia or soot.

Barley is a spring-sown crop for which the soil generally receives a more thorough cultivation, in consequence of which and of the rising temperature there will be sufficient nitrates produced for the needs of the crop, often more than enough when the barley follows a root crop that has been liberally manured and perhaps consumed on the ground by sheep. But being shallow rooted and having only a short growing season, the plant experiences a difficulty in satisfying its requirements for phosphoric acid, hence the necessary fertilizer consists in the main of this constituent. Only on sandy and gravel soils, exceptionally deficient in potash and subject to drought, is any benefit derived from a supply of potash to the barley crop.

A still more noteworthy example is provided by the swede turnip crop; the analysis of a representative yield would show it to withdraw from the soil about 150 pounds per acre of nitrogen, 30 pounds of phosphoric acid and 120 pounds of potash. Yet the ordinary fertilizer for the swede crop will consist in the main of phosphatic material with but a small quantity of nitrogen and rarely or never any potash; for example, 400 pounds of superphosphate or 500 pounds of basic slag according to the soil (*i. e.*, 50 to 100 pounds of phosphoric acid), together with 12 to 15 pounds of nitrogen as contained in 50 pounds of sulphate of ammonia will form a very satisfactory mixture. The swede is sown late in the season after a very thorough preparation of the soil, so that the nitrification alone of the nitrogenous residue in the soil is capable of furnishing almost all the large amount of nitrogen it requires; it is very shallow rooted and must be supplied with an abundance of phosphoric acid. It was considerations of this kind which led Ville to suggest that for each crop there is a "dominant" fertilizing constituent, *e. g.*, nitrogen for wheat, phosphoric acid for swedes, and that the particular dominant is the constituent which the plant finds the most difficulty in appropriating from the soil, and hence which is therefore

more often indicated by a comparative deficiency instead of abundance in the ash of the plant. Such a theory is, however, not borne out by more general experiments; many plants do not exhibit such idiosyncrasies as are shown by wheat and swedes but require a general fertilizer the composition of which is determined more by the soil than by the plant. Indeed, no theory of manuring can be based upon the plant alone, but must also take the soil into account, so that a fertilizer may be regarded as rectifying the deficiencies of the soil as far as regards the requirements of the crop in question. What those special requirements are can only be decided by experiment, just as the soil conditions are ascertainable by trial rather than from *a priori* considerations of analysis. If an analysis be made of any soil in cultivation it will be found to contain sufficient plant food for the nutriment of a hundred or more full crops: the soil of the unmanured plot on the Rothamsted wheat field contained in 1893, after 54 years' cropping without fertilizer, 2,570 pounds per acre of nitrogen, 2,950 pounds of phosphoric acid and 5,700 pounds of potash. Of course much of this material is in a highly insoluble condition, but though attempts have been made by the use of weak acid solvents to discriminate between the total plant food in the soil and that portion of it which may be regarded as available for the plant, no proper dividing line can be thus drawn. The availability of a given constituent, say of phosphoric acid, will depend, as has already been seen, upon the nature of the crop; a given soil may contain sufficient easily soluble phosphoric acid for the needs of the wheat plant and yet fail to supply swede turnips with what they require. Again, the mechanical texture of the soil may be such as to limit the root range of the plant, so that a richer soil is necessary to produce the same result as is obtained in a poorer soil of more open structure; the state of the microflora of the soil may also have much to do with the amount of a given nutrient which can reach the plant.

Perhaps the best general point of view of the action of fertilizers is obtained by extending the "law of the minimum" originally enunciated by Liebig, according to which the yield of a given crop will be limited by the amount of the one particular constituent which may happen to be deficient; if the soil, for example, is lacking in nitrogen the yield will be proportional to the supply of nitrogen in the fertilizer, and no excess of other constituents will make up for the shortage of nitrogen. To take an example from the Rothamsted experiments, the following table shows the yield of wheat,

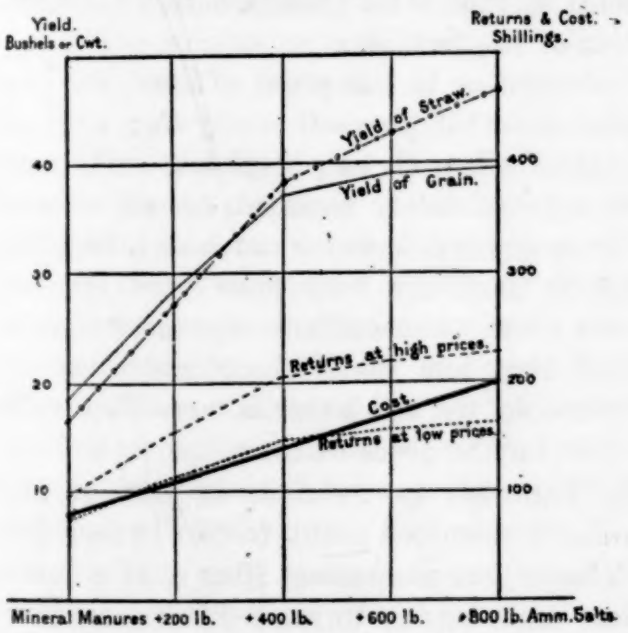
EXPERIMENTS ON WHEAT, BROADBALK FIELD, ROTHAMSTED

Averages over Thirteen Years (1852–64)

Plot	Manures per Acre	Dressed Grain		Straw	
		Produce per Acre	Increase for each Additional 43 lb. N in Manure	Produce per Acre	Increase for each Additional 43 lb. N in Manure
		Bush.	Bush.	Cwt.	Cwt.
3	Unmanured	15.6	—	14.6	—
5	Minerals alone	18.3	—	16.6	—
6	" and 43 lb. N as ammonium-salts	28.6	10.3	27.1	10.5
7	Minerals and 86 lb. N as ammonium-salts	37.1	8.5	38.1	11.0
8	Minerals and 129 lb. N as ammonium-salts	39.0	1.9	42.7	4.6
16	Minerals and 172 lb. N as ammonium-salts	39.5	0.5	46.6	3.9

grain and straw, from the unmanured plot and from a series of plots, all of which receive an excess of phosphoric acid, potash, etc., but varying amounts of nitrogen, ranging from 43 pounds to 172 pounds per acre. That the nitrogen was deficient is shown by the almost negligible increase

produced by the mineral constituents without nitrogen; from this point the increase of yield is roughly proportional to the supply of nitrogen, until it reaches an excessive amount. The table also illustrates the generalization which is familiar to economists under the name of "the law of diminishing returns"—that the first expenditure of fertilizer or other factor of improvement is the most effective, each succeeding application producing smaller and smaller returns, until a further addition causes no increase in the yield. If the cost of the fertilizer, added to a prime outlay of 80 shillings per acre for the cultivation, and the value of the returns in cash, are expressed in the form of a diagram, the law is clearly expressed by the series of curves in the figure, where the cost of production forms a straight line that is always intersected by the curves expressing the value of the returns, which begin by rising more rapidly than the cost of production, but



Relation between cost of production and returns with varying quantities of manure

tend to become horizontal. The point of intersection when profit ceases is nearer the origin the lower the range of prices obtainable for the crop, as shown by the two curves representing the returns at low and high prices, respectively; this demonstrates that the expenditure on fertilizers or anything else required by the crop must be reduced when prices of produce are low, or, as expressed by the late Sir John Lawes, high farming is not remedy for low prices.

Liebig's law of the minimum must, however, be extended to all the factors affecting the yield as well as to the supply of plant food, *e. g.*, to such matters as the supply of water, the temperature, the texture of the soil. Any one of these may be the determining factor which limits the yield, or two or more of them may act successively at different periods of the plant's growth. On poor soils the water supply is very often the limiting factor, on very open soils because the water actually drains away, on extra close soils because the root range is so restricted that the plant has but little water at hand and the movements of soil water to renew the supply are very slow; in either case for comparatively long periods the plant will be sure to have as much nutriment as is required for the small growth permitted by the water present. It is only when the water supply is sufficient that the resources of the soil as regards all or any of the constituents of a fertilizer are tested, and may become in their turn the limiting factors in the growth of the crop. Hence it follows that fertilizers may often be wasted on poor land, where growth is limited by the texture of the soil, by the water supply or some other factor hardly controllable by the farmer: it is a truism that poor land can not be converted into good by manuring and that fertilizers give the best returns when applied to a good soil.

One fundamental difficulty still remains in considering the action of fertilizers: it has already been pointed out that a soil by no means notably fertile may contain

enormous quantities of plant food, which is, however, combined in so insoluble a form as to reach the plant in quantities insufficient for the requirements of the crop; for example, a soil may contain 0.1 per cent., or 2,500 pounds per acre, of phosphoric acid and yet yield a very indifferent swede crop unless it be supplied with an additional dressing of 50 pounds per acre of soluble phosphoric acid. It is usually assumed that the effect of this phosphoric acid manuring is due to the soluble nature of the fertilizer, because of which the additional plant food is directly available for the crop. But a little consideration of the reaction set up in the soil will show how insufficient such a theory must be; the phosphoric acid is very rapidly precipitated within the soil, as is shown by the fact that on many soils it remains close to the surface for many years and is never washed out into the drains. Bearing in mind this precipitation of the phosphoric acid in an insoluble condition, Whitney and Cameron proceed to argue that previous to the addition of the fertilizer a certain amount of phosphoric acid exists in solution in the soil water, this amount being in equilibrium with the various phosphates of calcium, iron, aluminium, etc., making up the great store of phosphates in the soil. This particular state of equilibrium would be but little disturbed by the addition of the soluble fertilizer in quantities which are small compared with the great mass of undissolved phosphates in contact with the soil water; the added phosphoric acid would only displace an almost equivalent amount of the phosphoric acid already in solution, and the concentration of the new solution would only differ from the old in the same degree as the ratio the phosphoric acid in the soil plus fertilizer (2,500 + 50 pounds of phosphoric acid) bears to the phosphoric acid originally in the soil (*i. e.*, 2,500 pounds phosphoric acid). In other words, before the fertilizer was added the soil water was as fully saturated with phosphoric acid as the amount of calcium, iron, aluminium and other bases would permit, and as these bases are present in enormous excess, the soil water must remain at the same saturation point after the fertilizer has been added, just as water will only hold 35 per cent. of the common salt in solution with however large a quantity of salt it may be in contact. Just in the same way the soil contains certain double silicates of which potassium is a constituent and these hydrolize to a slight extent in contact with the soil water, to yield a solution containing potassium ions. The addition of a soluble potassium salt, as in a fertilizer, will diminish the dissociation and therefore the solubility of the double silicate, the potassium of which is thrown out of solution until, as Whitney and Cameron argue, no more potassium ions remain in solution than were present before the addition of the fertilizer.

According to this point of view, the concentration of the soil water for a given plant food, such as phosphoric acid, must be approximately constant for all soils of the same type, however much or little phosphatic fertilizer may have been applied, and since water-culture experiments show that this low limit of concentration attained by the soil water is more than sufficient for the needs of the plant, no soil can be regarded as deficient in this or any other element of plant food. It therefore follows that the action, if any, of a fertilizer must be due to some other cause than the direct supply of plant food, with which the soil water must always be saturated to a degree which is quite unaffected by the supply of fertilizer.

This view of the interactions between the sparingly soluble phosphates of the soil, the soil water and the added soluble fertilizer,

can hardly be regarded as valid in theory, even if the conditions under which the reagents exist in the soil are the same as those which prevail in the laboratory when such conditions of equilibrium between sparingly soluble solids and water are worked out. It has no bearing whatever on the amount of nitrates in the soil water, since they come into a dissolved state as fast as the nitrifying bacteria produce them, and are not in equilibrium with any store of undissolved nitrates in the background. As regards phosphoric acid the theory assumes such an excess of bases that all soils behave alike and immediately precipitate the phosphoric acid, in practically the same form; while as regards potash the argument seems to forget that though the addition of a soluble potassium salt may throw some of the other sparingly soluble potassium compounds out of solution, the total amount of potassium remaining in solution is still greatly increased. The function of the carbonic acid in the soil water is ignored, as again the fact that the processes of solution in the soil must be in a constant state of change, so that it is the dynamic rather than the static solubility which is of importance. The soil is too complex a mixture to permit as yet of attaching great weight to theoretical deductions as to the actions taking place in it, and that the state of affairs postulated by Whitney and Cameron does hold in the soil has not however, been verified by experiment; the analyses, given by the authors of the theory, of the cold water extracts from a number of soils show great variations in their concentration, in nitrates, phosphoric acid and potash; nor is any evidence forthcoming that such concentrations are not immediately raised by the addition of fertilizers. Indeed, when the Rothamsted soils, with their long-continued difference in fertilizer treatment, are extracted with water charged with carbon dioxide, the nearest laboratory equivalent to the actual soil water, the amount of phosphoric acid going into solution is closely proportional to the previous fertilizer supply, and this proportionality is maintained if the extraction is repeated with fresh solvent, as must be the case in the soil. In the field it is not merely the initial concentration of the soil water in plant food which determines the supply of nutriment to the crop, it is also the capacity of the soil to keep renewing the solution as the plant withdraws from it the essential elements.

In one essential respect again the conditions prevailing in the soil are very different from those of the laboratory; in the soil all reactions are extremely localized, since they take place in the thin film of water normally surrounding the soil particles, in which movement of the dissolved matter takes place very slowly and mainly by diffusion. Of the extreme slowness of the diffusion of soluble salts in the soil the Rothamsted experiments afford some good examples; for instance, on the grass plots only an imaginary line divides the pots receiving different fertilizers, the manure is sown right up to the edge of the plot, a screen being placed along the edge to prevent any being thrown across the boundary, then immediately on the other side of the boundary the different treatment begins. In two cases plots receiving very large amounts of soluble fertilizer, *e. g.*, 550 pounds per acre of nitrate of soda or 600 pounds per acre of ammonium salts, march with plots receiving either no fertilizer or a characteristically different one, yet in neither case is there any sign in the herbage of the soluble fertilizer having diffused over the boundary. Although the treatment has been repeated now for 52 years, the dividing line between the two plots remains perfectly sharp and the rank herbage produced by the excess of nitro-

genous fertilizer on one side does not stray six inches over the boundary. Again, on the Rothamsted wheat field the plots are 20.7 feet in breadth and were separated by unfertilized strips only about a foot in breadth; in 1893 each plot was sampled down to a depth of 7.5 feet and the amount of nitrates was determined in each successive sample of nine inches in depth. The amount of nitrates found was in each case characteristic of the supply of nitrogen to the surface of the plot, and right down to the lowest depth there were no signs of the proportions approximating to a common level, as they would have done had any considerable amount of lateral diffusion been taking place. Considering that the plots are only separated by a foot or so of soil and each had been receiving its particular amount of nitrogen for forty and in some cases for fifty years, the sharp differentiation of plot from plot in the amount of nitrates at a depth of seven feet is sufficiently remarkable and is evidence that the movements of the soluble salts in the soil are confined to up and down motions due to percolation and capillary uplift, and take place laterally only to an insignificant extent.

From these considerations we may conclude that when a fertilizer is mixed with the soil each particle will establish round itself a zone of a comparatively concentrated solution to which the plant's roots will be drawn by the ordinary chemiotactic actions, and that these zones will extend but a little way into the generally much less dilute mass of the soil water, because of the slowness of the diffusion process.

That some such state of things prevails in the soil may be surmised from the common farming experience of the benefits derived from sowing the fertilizer close to the seed in such a case as superphosphate and turnip seed, where the fertilizer is not injurious to germination and the young plant is specially dependent on being rapidly pushed into growth in the early stages. Again, the intimate way in which the feeding fibrous roots of a plant will surround and cling to a fragment of fertilizer in the soil, such as a bone or a piece of shoddy, shows that some other actions are at work in the soil than the mere feeding of the plant upon the nutrients contained in the soil solution.

Whitney and Cameron's theory also supposes that the plant itself exerts no solvent action, whereas it has often been supposed that the roots excrete substances of an acid nature which exert a solvent action upon the soil particles. In this direction an experiment of Sachs's has become classical: he took a slab of polished marble and set it vertically in a pot of soil in which beans or some kindred plants were grown. After the plants had been growing for some time the contents of the pot were turned out and the slab of marble washed, whereupon the polished surface was found to be etched wherever the roots had been growing in contact with it. A polished slab of gypsum similarly treated shows a raised pattern wherever the roots have protected the surface from the solvent action of the general mass of water in the soil. Although Sachs himself attributed the etching to the action of the carbon dioxide which is always being given off by the roots, it has also been set down to fixed acids excreted by the root hairs, and determinations have been made of the acidity of the sap of the roots with the idea of differentiating between the solvent power of various plants. The roots of germinating seedlings are also found on occasion to redden blue litmus paper and undoubtedly may excrete substances of an acid character, but the behavior of seedlings, which are building up their fresh tissue out of the broken-down reserve materials contained in the seed, is essen-

tially different from that of plants leading an independent existence, so that nothing is thereby proved as to the source of the etching in Sachs's experiments.

Czapek instituted a fresh series of experiments with smooth slabs prepared by floating on to glass plates mixtures of plaster of paris and various phosphates of calcium, iron and aluminium; since only the calcium phosphate was etched, most of the possible acids were excluded and the etching action of the plant's roots was restricted to carbon dioxide or acetic acid. This latter was again excluded by a further experiment in which the slab was colored with congo red, and as this was not affected, the sole remaining solvent body the plant could have excreted was carbon dioxide. Again it has already been shown that water cultures containing nitrates, where the plant is growing in such solutions as exist under normal soil conditions, tend to become alkaline instead of acid, so that the balance of evidence is against the idea that plant roots excrete any fixed acids exerting a solvent action upon the soil particles. The carbon dioxide, however, probably exerts a considerable action, especially in the immediate vicinity of the root from which it is given off, for as it passes through the cell wall it must momentarily form a solution of considerable concentration, possessing a proportionally increased solvent power, and it is to this supersaturated solution that may be attributed the highly localized attack of the roots upon the soil particles. An experiment of Kossowitsch's illustrates the part played by the roots in attacking the insoluble materials in the soil: two pots of sand were prepared, each mixed with the same quantity of calcium phosphate in the form of ground rock phosphate; a third pot contained sand only. In this latter, and in one of the pots containing the calcium phosphate, seeds of mustard, peas and flax were sown. The growing plants were then furnished with a slow continuous supply of water containing appropriate amounts of nitrates, potash and other nutrient salts except phosphates. Before, however, this nutrient solution reached the pot containing the sand only it was made to percolate through the second pot containing sand and calcium phosphate, but it was applied directly to the pot containing calcium phosphate. In the pot containing calcium phosphate the growth was much greater than in the other pot, where the nutrient solution only contained what phosphoric acid it could dissolve in its passage over the calcium phosphate in the pot where nothing was growing, although this solution was continually renewed. The only factor determining the supply of phosphoric acid and the consequent difference in growth was the solvent action of the roots where they were actually in contact with the calcium phosphate, and this solvent action, as has already been shown, may most probably be attributed to the carbon dioxide excreted by the roots.

Following up their conclusions that the soil water possesses an approximately constant composition under all circumstances and always contains more of the constituents of plant food than would be required for the nutrition of the plant, Whitney and his colleagues have suggested another theory of fertilizer action. According to this point of view a soil falls off in fertility and ceases to yield normal crops, not because of any lack of plant food brought about by the continuous withdrawal of the original stock in the soil, but because of the accumulation of injurious substances excreted from the plant itself. These toxins are specific to each plant, but are gradually removed from the soil by processes of decay, so that if a proper rotation of crops be practised, to ensure that

the same plant only recurs after an interval long enough to permit of the destruction of its particular self-formed toxin, its yield will be maintained without the intervention of fertilizers. The function of fertilizers is to precipitate or to put out of action these toxins, and various bodies such as lime, green manure and ferric hydrate are also effective in this direction; the same result of destruction of the toxins excreted by the plant may even be brought about by minute quantities of certain bodies like pyrogallol. According to this theory the function of fertilizers is to remove toxins rather than to feed the plant; they are only required when the same crop is grown continuously and the need for them may be obviated by a judicious rotation which permits of the destruction of the toxins by natural causes. Careful consideration will show that this theory can be made to fit a good many of the phenomena of plant nutrition; it would also explain the difficulties experienced in growing certain crops continuously on the same ground; it is, in fact, an elaborated revival of one of the earliest explanations of the value of rotations, originally suggested by de Candolle. Furthermore, Whitney's colleagues have succeeded in extracting certain substances from the soil—oxystearic acid, pyridin derivatives, tyrosin, etc., which when introduced into water cultures are toxic to seedling plants. The compounds isolated are, however, all of them products of the oxidation and decay of proteins, fats and other compounds contained in plant residues; there is no evidence to show that they are specific excretions from particular plants or that they are more abundant in soil impoverished by the growth of a particular crop than in soil which would be usually termed rich. Again, it has not been demonstrated that such substances, although harmful to young plants in water culture, are toxic under soil conditions; it is well known how exceedingly sensitive are plants in water culture, where growth, for example, is inhibited by traces of copper not to be detected by ordinary methods of analysis. A body like ammonia, itself a product of protein decay and present in the soil, is exceedingly toxic to water cultures, yet when applied to the soil it increases the growth of the plant. Turning to the fertilizer side of the theory, evidence is yet lacking to show that fertilizers in such dilute solutions as they form in the soil water can exert any precipitating or destructive action on such toxic substances as have been extracted from the soil; particularly it is the specific action of fertilizers which is difficult to explain. Why should substances so dissimilar as nitrate of soda and sulphate of ammonia exert the same sort of action on the same toxin? Why should phosphates cause all classes of plants to develop in one direction, or why should it be appropriate to the toxins of all plants on one particular type of soil, whereas potash answers on another soil type? Lastly, there is a lack of evidence for the fundamental thesis that the rotation will take the place of fertilizers and that the yield only falls off when a particular crop is grown continuously on the same land.

On the rotation field at Rothamsted the yield of wheat on the unfertilized plot has been remarkably maintained; for the last five courses (tenth to fourteenth of the whole series) it has averaged 26.2 bushels per acre, but it is below the yield of the fertilized plots on the Broadbalk field, which averaged 35.7, 32 and 39.7 bushels for the same years, and also below the fertilized plot on the same rotation field, which averaged for the same period 37.1 bushels per acre, although the fertilizer is only applied once in four years to the swedes, which are followed by barley and either clover or a bare fallow before the turn of the wheat comes round. But with

other crops than wheat no such maintenance of yield is to be seen on the unfertilized plot of the rotation field—the barley yield has been reduced to 15.8 bushels against 27.7 on the fertilized plot, the clover yield to 940 pounds against 3,780 pounds on the fertilized plot, and the turnips to as little as 1,600 pounds against 40,000 pounds on the fertilized plot. Here we see that with the barley, clover, and particularly with the turnip crop, a rotation is quite unable to do the work of the fertilizer, the yield of turnips is reduced to a minimum on the impoverished soil, even though the crop only comes round once in four years and then grows so poorly that it can do little specific excretion to harm the succeeding crop. Many instances could be given of the incapacity of certain plants to grow in soil the fertility of which had been exhausted by other crops; for example, at Rothamsted in 1903 swede turnips were sown on Little Hoos field, which was known not to have been cropped with swedes or any kindred crop for more than forty years—and the average yield from thirty-two unmanured plots was only 9.3 tons per acre, although an exceptionally good start was made by the plant. In the following season barley was grown and the unmanured plots averaged 24.2 bushels per acre, a relatively much higher yield than the swedes had shown—yet barley had been repeatedly grown on the field in the years immediately before it was brought under experiment.

As it stands at present Whitney's theory must be regarded as lacking the necessary experimental foundation, no convincing evidence has been produced of the fundamental fact of the excretion of toxic substances from plants past the autotrophic seedling stage, nor is there direct proof of the initial supposition, that all soils give rise to soil solutions sufficiently rich in the elements of plant food to nourish a full crop did not some other factor come into play. If, however, we give the theory a wider form, and, instead of excretions from the plant, understand débris of any kind left behind by the plant and the results of bacterial action upon it, we may thereby obtain a clue to certain phenomena at present imperfectly understood. The value of a rotation of crops is undoubted and in the main is explicable by the opportunity it affords of cleaning the ground, the freedom from any accumulation of weeds, insect or fungoid pests associated with a particular crop, and to the successive tillage of different layers of the soil, but for many crops there remains a certain beneficial effect from a rotation beyond the factors enumerated.

The Rothamsted experiments have shown that wheat can be grown continuously upon the same land for more than fifty years and that the yield when proper fertilizers are applied remains as large in the later as in the earlier years of the series, any decline that is taking place is hardly outside the limits of seasonal variation and can easily be accounted for by the difficulties of tillage and the increase of one or two troublesome weeds. Mangolds again in the Rothamsted experiments show no falling off in yield, though they have now been grown upon the same land for thirty-two years, but with the barley crop, despite the application of fertilizers, there is a distinct secular decline in the yield. Again, it was found impossible to obtain satisfactory crops of swede turnips upon the same land for more than ten or twelve years in succession, and clover is well known to render the land "sick" for its own renewed growth for a period of from four to eight years on British soil. In this last case the persistence of the resting stages of the *sclerotinia* disease in the land may be the determining factor, but there are other crops, *e. g.*, flax, hemp and strawberries, which are considered by the practical cultivator to render the land more or less "sick," so that their

growth can not profitably be renewed until an interval of some years has elapsed.

Again it is well known that when a plant is sown upon land which has not carried that particular crop for many years beforehand it starts into growth with a vigor it rarely displays upon land where it forms an item in the regular rotation, even though the new land is so impoverished that the final yield is indifferent. In the instance quoted above, where swedes were sown on Little Hoos field, Rothamsted, after a very long interval, although the yield was poor on the unmanured plots, yet the seeds germinated and made their early growth in a very remarkable fashion, incomparably better than did the same seed sown upon adjoining land in a high state of fertility, but which had been cropped with swedes from time to time previously. There is thus some positive evidence that most plants—some to a very slight degree, like wheat and mangolds, others markedly, like clover, turnips, and flax—effect some change in the soil which unfits it for the renewed growth of the crop. The injurious action may even arise from the growth of a different crop, as in the well-known experiments at the Woburn Fruit Farm, where Pickering has shown that the roots of grasses exert a positively injurious effect, distinct from competition for food, water or air, upon fruit trees growing in the same soil.

Assuming that the persistence in the soil of obscure diseases appropriate to the particular plant can be neglected as the cause of these phenomena, there still remains some unexplained factor arising from a plant's growth which is injurious to a succeeding crop, and this may either be the excreted toxins of Whitney's theory or may be some secondary effects due to the competition of injurious products of the bacteria and other microflora accumulating in the particular soil layer in which the roots of the crop chiefly reside.

Experimental evidence is as yet wanting as to these highly complex interactions between the higher plants and the microflora of the soil, but Russell and other observers have shown how greatly a disturbance of the normal equilibrium of the flora of the soil may affect its fertility, as measured by the yield of a higher plant. Partial sterilization, such as is brought about by heating the soil to 98° for ten hours, will double the yield of the succeeding crop and will show a perceptible beneficial effect up to the fourth crop after the heating, and exposure to the vapors of volatile antiseptics like toluene or carbon bisulphide, which are afterwards entirely removed by exposure, will increase the yield in a similar but smaller degree; even drying the soil appears to have an influence upon its fertility.

It is in this direction, perhaps, that the clue may be found to the unexplained benefits of the rotation of crops, and to some of the other facts difficult of explanation upon the ordinary theories of plant nutrition, which have been advanced by Whitney and his co-workers. The soil, however, is such a complex medium—the seat of so many and diverse interactions, chemical, physical and biological—and is so unsusceptible of synthetic reproduction from known materials, that experimental work of a crucial character becomes extremely difficult and above all requires to be interpreted with extreme caution and conservatism.

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THE DUBLIN MEETING OF THE BRITISH ASSOCIATION—II.

The Metabolism of the Plant Considered as a Catalytic Reaction[1]

After outlining the three fundamental principles of reaction-velocity, the law of

[1] Address by Professor F. F. Blackman, M.A., F.R.S., president of the botanical section, on "The Manifestations of the Principles of Chemical Mechanics in the Living Plant."

mass, and the catalytic acceleration of reaction velocity, Professor Blackman proceeded to consider the broad phenomena of metabolism or chemical change in the living organism from the point of view of these principles of chemical mechanics.

Plants of all grades of morphological complexity, from bacteria to dicotyledons, have this in common, that throughout their active life they are continually growing. Putting aside the *qualitative* distribution of growth that determines the morphological form, as a stratum of phenomena above the fundamental one that we are about to discuss, we find that this growth consists in the assimilation of dead food-constituents by the protoplasm, with a resulting increase in the living protoplasm accompanied by the continual new formation of dead constituents, gaseous CO_2, liquid water, solid cellulose, and what not. This continual flux of anabolism and katabolism is the essential character of metabolism, but withal the protoplasm increases in amount by the excess of anabolism over katabolism.

Protoplasm has essentially the same chemical composition everywhere, and in the whole range of green plants the same food materials seem to be required; the six elements of which proteids are built are obviously essential in quantity as building material, but in addition small amounts of Fe, Ca, K, Mg, Na, Cl and Si are in some other way equally essential. What part these secondary elements play is still largely a matter of hypothesis.

Regarding metabolism thus crudely as if it were merely a congeries of slow chemical reactions, let us see how far it conforms to the laws of chemical mechanics we have outlined.

If the supply of any one of these essential elements comes to an end, growth simply ceases and the plant remains stationary, half-developed. If a *Tropæolum* in a pot be watered with dilute salt-solution, its stomata soon close permanently, and no CO_2 can diffuse in to supply the carbon for further growth of the plant. In such a condition the plant may remain for weeks looking quite healthy, but its growth may be quite in abeyance.

In agricultural experience, in manuring the soil with nitrogen and the essential secondary elements, the same phenomenon is observed when there is a shortage of any single element. If a continuous though inadequate supply of some one element is available, then the crop development is limited to the amount of growth corresponding to this supply. Agriculturalists have formulated the "law of the minimum," which states that the crop developed is limited by the element which is minimal, *i. e.*, most in deficit. Development arrested by "nitrogen-hunger" is perhaps the commonest form of this. All this is, of course, in accordance with expectation on physical-chemical principles. The quantity of anabolic reaction taking place should be proportional to the amount of actively reacting substances present, and if any one essential substance is quite absent the whole reaction must cease. It therefore seems clouding a simple issue and misleading to say of a plant which, from the arrested development of nitrogen-hunger, starts growth again when newly supplied with nitrogen that this new growth is a response to a "*nitrogen stimulus.*" It would appear rather to be only the removal of a limiting condition.

Let us now move on a stage. Suppose a growing plant be liberally supplied with all the thirteen elements that it requires, what, then, will limit its rate of growth? Fairy bean-stalks that grow to the heavens in a night elude the modern investigator, though some hope soon to bring back that golden age with overhead electric wires and underground bacterial inoculations. If

everything is supplied, the metabolism should now go on at its highest level, and quantities of carbon, nitrogen, hydrogen and oxygen supplied as CO_2, nitrates and water will interact so that these elements become converted into proteid, cellulose, etc. Now this complex reaction of metabolism only takes place in the presence of protoplasm, and a small amount of protoplasm is capable of carrying out a considerable amount of metabolic change, remaining itself undestroyed. We are thus led to formulate the idea that metabolism is essentially a catalytic process. In support of this we know that many of the inherent parts of the protoplasmic complex are catalytic enzymes, for these can be separated out of the protoplasm, often simply by high mechanical pressure. We know, too, nowadays that the same enzymes that accelerate katabolic processes also accelerate the reverse anabolic processes.

In time a small mass of protoplasm will, while remaining itself unchanged, convert many times its own weight of carbon from, let us say, the formaldehyde (HCHO) of photosynthesis to the carbon dioxide (CO_2) of respiration.

If metabolism is a complex of up-grade and down-grade changes catalyzed by protoplasm we must expect the amount of metabolism to obey the law of mass and to be proportional to the masses of substances entering into the reaction. The case when any one essential element is a limiting factor we have already considered. When all are in excess, then the *amount of the catalyst present* becomes in its turn the limiting factor. Transferring this point of view to the growing plant, we expect to find the limited mass of protoplasm and its constituent catalysts setting a limit to the rate of metabolic change in the extreme case where all the materials entering into the reaction are in excess. When once this supply is available further increase in supplies can not be expected to accelerate the rate of growth and metabolism beyond the limit set by the mass of protoplasm. This, of course, is in accordance with common experience. The clearest experimental evidence is in connection with respiration and the supply of carbohydrates—this, no doubt, because the carbohydrate material oxidized in respiration is normally stored inside plant-cells in quantity and can be estimated. When the supplies for an internal process have to be obtained from outside, then we have the complications of absorption and translocation to obscure the issue, especially in the case of a higher plant.

Let us first take a case where the carbohydrate supply is in excess and the amount of catalytic protoplasm is small and increasing. Thus it is in seeds germinating in the dark: respiration increases day by day for a time, though carbohydrate reserves are steadily decreasing. Palladine[2] has investigated germinating wheat by analyzing the seedlings and determining the increase of the essential (non-digestible) proteids day by day. The amount of these proteids he regards as a measure of the amount of actual protoplasm present. Assuming this to be so, he finds an approximately constant ratio between the amount of protoplasm at any stage and the respiration.

As germination progresses in the dark the supplies of reserve carbohydrate presently fail, and then the respiration no longer increases in spite of the abundant protoplasm. According to our thesis the catalyst is now in excess and the CO_2 production is limited by the shortage of respirable material.

This second type was more completely investigated by Miss Matthæi and myself in working on the respiration of cut leaves of cherry-laurel kept starved in

[2] *Revue gén. de botanique,* Tome VIII., 1896.

the dark. For a time the CO_2 production of these non-growing structures remains uniform, and then it begins to fall off in a logarithmic curve. We interpret both phenomena in the same way: in the initial level phase the respirable material in the leaf is in excess, and the amount of catalytic protoplasm limits the respiration to the normal biological level; in the second falling phase some supply of material is being exhausted, and we get a logarithmic curve controlled by the law of mass, as much, it would seem, as when cane-sugar is hydrolyzed in aqueous solution.

After these two illustrations of the action of the law of mass from the more simple case of respiration we return to the consideration of the totality of metabolic reactions as exemplified in growth.

What should we expect to be the ideal course of growth, that is, the increase of the mass of the plant regarded as a complex of reactions catalyzed by protoplasm? Let us consider, first, the simplest possible case, that of a bacterium growing normally in a rich culture solution. When its mass has increased by anabolism of the food material of the culture medium to a certain amount, it divides into two. As all the individuals are alike; counting them would take the place of weighing their mass. The simplest expectation would be that, under uniform conditions, growth and division would succeed each other with monotonous regularity, and so the number or mass of bacteria present would double itself every n minutes. This may be accepted as the ideal condition.

The following actual experiment may be quoted to show that for a time the ideal rate of growth is maintained, and that at the end of every n minutes there is a doubled amount of protoplasm capable of catalyzing a doubled amount of chemical change and carrying on a doubled growth and development.

From a culture of *Bacillus typhosus* in broth at 37° C. five small samples were withdrawn at intervals of an hour, and the number of bacteria per unit volume determined by the usual procedure. The number of organisms per drop increased in the following series: 6.7, 14.4, 33.1, 70.1, 161.0.[3] This shows a doubling of the mass of bacteria in every fifty-four minutes and actually represents a strictly logarithmic curve.

We may quote some observations made by E. Buchner[4] of the rate at which bacteria increase in culture media. Bacillus coli communis was grown at 37° C. for two to five hours, and by comparison of the initial and final numbers of bacteria the time required for doubling the mass was calculated. Out of twenty-seven similar experiments a few were erratic, but in twenty cases the time for doubling was between 19.4 and 24.8 minutes, giving a mean of 22 minutes. This produces an increase from 170 to 288,000 in four hours. No possible culture medium will provide for prolonged multiplication of bacteria at these rates.

Cohn[5] states that if division takes place every sixteen minutes, then in twenty-four hours a single bacterium 1 μ long will be represented by a multitude so large that it requires twenty-eight figures to express it, and placed end to end they would stretch so far that a ray of light to travel from one end to the other would take 100,000 years. The potentialities of protoplasmic catalysis are thus made clear, but the actualities are speedily cut short by limiting factors.

[3] For this unpublished experiment on bacterial growth I am indebted to Miss Lane-Claypon, of the Lister Institute of Preventive Medicine.

[4] Buchner, *Zuwachsgrossen u. Wachsthumsgeschwindigkeiten*, Leipzig, 1901.

[5] Cohn, *Die Pflanze*, Breslau, 1882, p. 438.

For a while, however, this ideal rate of growth is maintained. At the end of every n minutes there is a doubled amount of protoplasm present, and this will be capable of catalyzing twice the amount of chemical change and carrying on a doubled amount of growth and development. This is what common sense and the law of mass alike indicate, and is exactly what the logarithmic curve expresses.

This increase of the amount of catalytic protoplasm by its own catalytic activity is an interesting phenomenon. In Section K we call it growth, attribute it to a specific power of protoplasm for assimilation (in the strict sense), and leave it alone as a fundamental phenomenon, but are much concerned as to the distribution of the new growth in innumerable specifically distinct forms. In the chemical section they call this class of phenomenon "autocatalysis," and a number of cases of it are known. In these a chemical reaction gives rise to some substance which happens to catalyze the particular reaction itself, so that it goes on and on with ever-increasing velocity. Thus, we said that free acid was a catalyst to the hydrolysis of cane-sugar; suppose now that free acid were one of the products of the hydrolysis of sugar, then the catalyst would continually increase in amount in the test-tube, and the reaction would go faster and faster. Under certain conditions this actually happens. Again, when methyl acetate is hydrolyzed we normally get methyl alcohol and free acetic acid. This free acid acts as a catalyst to the hydrolysis, and the rate of change continually accelerates. Here, if the supply of methyl acetate were kept up by constant additions, the reaction would go faster and faster with a logarithmic acceleration.

For a clear manifestation of this autocatalytic increase in the plant it is, of course, essential that the supply of food materials to the protoplasm be adequate.

ACCELERATION OF REACTION-VELOCITY BY TEMPERATURE

We now turn to consider the fourth and last of the principles of chemical mechanics which we might expect to find manifested in metabolism.

It is a universal rule that rise of temperature quickens the rate at which a chemical reaction proceeds. Of course in some rare conditions this may not be obvious, but be obscured by superposed secondary causes; but almost always this effect is very clearly marked.

Further, the nature of the acceleration is a peculiar one. Rise of temperature affects nearly all physical and chemical properties, but none of these is so greatly affected by temperature as is the velocity of chemical reaction. For a rise of 10° C. the rate of a reaction is generally increased two or three fold, and this has been generalized into a rule by van't Hoff. As this increase is repeated for each successive rise of 10° C. either by the same factor or a somewhat smaller one, the acceleration of reaction-velocity by temperature is logarithmic in nature, and the curve representing it rises ever more and more steeply. Thus keeping within the vital range of temperature a reaction with a temperature factor of $\times 2$ per 10° C. will go sixteen times as fast at 40° C. as at 0° C., while one with a factor of $\times 3$ will go eighty-one times as fast.

This general law of the acceleration of reactions by temperature holds equally for reactions which are being accelerated by the presence of catalysts. As we regard the catalyst as merely providing for the particular reaction it catalyzes, a quick way round to the final stage by passing through the intermediate stage of forming a temporary addition-compound with the catalyst itself, so we should expect rise of temperature to accelerate similarly these substituted chemical reactions.

If this acceleration is a fundamental

principle of chemical mechanics it is quite impossible to see how vital chemistry can fail to exhibit it also.

ACCELERATION OF VITAL PROCESSES BY TEMPERATURE

At present we have but a small number of available data among plants to consider critically from this point of view. But all the serious data with which I am acquainted, which deal with vital processes that are to be considered as part of the protoplasmic catalytic congeries, do exhibit this acceleration of reaction-velocity by temperature as a primary effect.[6]

Let us briefly consider these data. On the katabolic side of metabolism we have the respiratory production of CO_2, and opposed to it on the anabolic side the intake of carbon in assimilation.

As a measure of the rate of the metabolic processes constituting growth we have data upon the division of flagellates; and finally there is the obscure process of circulation of protoplasm.

The intensity of CO_2 production is often held to be a measure of the general intensity of metabolism, but any relation between growth-rate and respiration has yet to be clearly established. Our science is not yet in the stage when quantitative work in relation to conditions is at all abundant; we are but just emerging from the stage that chemistry was in before the dawn of physical chemistry.

Taken by itself the CO_2-production of an ordinary green plant shows a very close relation with temperature. In the case of the cherry-laurel worked out by Miss Matthæi and myself the respiration of cut leaves rises by a factor of 2.1 for every 10° C. This has been investigated over the range of temperatures from 16° C. to 45° C. At this higher temperature the leaves can only survive ten hours in the dark, and their respiration is affected in quite a short time, but in the initial phases the CO_2 output has the value of .0210 gr. per hour and unit weight of leaf, while at 16°.2 C. the amount is only .0025 gr. CO_2. Thus the respiration increases over a range of tenfold with perfect regularity solely by increase of temperature. No reaction in a test-tube could show less autonomy. At temperatures above 45° C. the temperature still sooner proves fatal unless the leaf is illuminated so as to carry out a certain amount of photosynthesis and compensate for the loss of carbon in respiration. Thus, with rising temperature, there is at no time any sign of an optimum or of a decrease of the intensity of the *initial* stage of respiration.

Here, then, on the katabolic side of metabolism we have no grounds for assuming that "temperature-stimuli" are at work regulating the intensity of protoplasmic respiration, but we find what I can only regard as a purely physical-chemical effect. The numbers obtained by Clausen[7] for the respiration of seedlings and buds at different temperatures indicate a temperature coefficient of about 2.5 for a rise of 10° C.

To this final process of katabolism there could be no greater contrast than the first step of anabolism, the assimilation of carbon by the protoplasm as a result of photosynthesis. We must, therefore, next inquire what is the relation of this process to temperature.

This question is not so simple, as leaves can not satisfactorily maintain the high rate of assimilation that high temperatures allow. The facts of the case were clearly

[6] A collection of twenty cases, mostly from animal physiology, by Kanitz (*Zeits. für Elektrochemie*, 1907, p. 707), exhibits coefficients ranging from 1.7 to 3.3.

[7] *Landwirtschaftliche Jahrbücher*, Bd. XIX., 1890.

worked out by Miss Matthæi,[8] the rate of assimilation by cherry-laurel leaves being measured from — 6° C. to + 42° C. Up to 37° C. the curve rose at first gently and then more and more steeply, but on calculating out the values it is found that the acceleration for successive rises of 10° C. becomes less and less. Between 9° C. and 19° C. the increase is 2.1 times, the highest coefficient measured, and exactly the same coefficient as for respiration in this plant, which in itself is a striking point, seeing how different the processes are. The decrease of the coefficient with successive rises is a state of things which is quite general among non-vital reactions. A critical consideration of the matter leads one to the conclusion, however, that this failure to keep up the temperature acceleration is really due to secondary causes, as is also the appearance of an optimum at about 38° C. Some of these causes have been discussed by me elsewhere,[9] and I hope to bring a new aspect of the matter before the section in a separate communication. The conclusion formerly come to was that probably in its initial stages assimilation at these very high temperatures started at the full value indicated by a theoretically constant coefficient, but that the protoplasm was unable to keep up the velocity, and the rate declined. It must be borne in mind here that quite probably no chloroplast since the first appearance of green cells upon the earth had ever been called upon for anything like such a gastronomic effort as these cherry-laurel leaves in question. It is not to be wondered that their capacities speedily declined at such a banquet, and that the velocity-reaction of anabolic synthesis traces a falling curve in spite of the keeping up of all the factors concerned, to wit, temperature, illumination and supply of CO_2. This decline is not permanent, but after a period of darkening the power of assimilation returns. Physical-chemical parallels can easily be found among cases where the accumulation of the products of a reaction delays the apparent velocity of the reaction, but this complicated case may be left for further research.

In relation to assimilation, then, we must say that owing to secondary causes the case is not so clear over the whole range of temperature as that of respiration, but that at medium temperatures we have exactly the same relation between reaction-velocity and temperature.

We may consider now some data upon the combined net result of anabolic and katabolic processes. Such total effects are seen in their clearest form among unicellular saprophytic organisms for which we have a few data. Mlle. Maltaux and Professor Massart[10] have published a very interesting study of the rate of division of the colorless flagellate *Chilomonas paramœcium* and of the agents which they say stimulate its cell-division, in particular alcohol and heat.

They observed under the microscope the time that the actual process of division into two took at different temperatures. From 29 minutes at 15° C. the time diminished to 12 minutes at 25° C., and further to 5 minutes at 35° C. The velocities of the procedure at the three temperatures 10° C. apart will therefore be in the ratio of 1 : 2.4 : 5.76, which gives a factor of 2.4 for each rise of 10° C.

Now we are told by the investigators that at 35° C. Chilomonas is on the point of succumbing to the heat, so that the division rate increases right up to the death point, with no sign of an optimum effect.

[8] *Phil. Trans. Roy. Soc.*, Ser. B, Vol. CXCVII., 1904.

[9] "Optima and Limiting Factors," *Annals of Botany*, Vol. XIX., April, 1905.

[10] Maltaux and Massart, *Recueil de l'Institut botanique Bruxelles*, Tome VI., 1906.

Below 14° C. no observations are recorded.

Here, then, we have throughout the whole range exactly the same primary temperature relation exhibited by the protoplasmic procedure that we should expect for a chemical reaction in a test-tube.

This division phase is only a part of the life-cycle of the flagellate, and between division it swims about anabolizing the food material of the medium and growing to its full size ready for the next division. One wishes at once to know what is the effect of the temperature upon the length of the life-cycle. Is the whole rate of metabolism quickened in the same way as the particular section concerned with actual division? Of course a mobile flagellate can not be followed and its life-cycle directly timed, but the information was obtained by estimating carefully what percentage of individuals were in a state of actual division at each temperature. It was found that always 4 per cent. were dividing, whatever the temperature. This proves that the whole life-cycle is shortened in exactly the same proportion as the process of division at each temperature, and that it is just twenty-five times as long. Therefore, the life-cycle is 125 mins. at 35° C. and 725 mins. at 15° C., so that here, again, we have the physical-chemical relation with a factor of 2.4 for each rise of 10° C.

In this paper of Maltaux and Massart these relations are not considered as the manifestation of physical-chemical principles, but are regarded as reactions to stimuli; and the paper contains a number of experiments upon the effect of sudden changes of temperature upon the occurrence of division. As far as one can make out from inspection of the scattered literature, it does seem established that sudden changes of temperature act as stimuli in the strict sense of the word. In many investigations one finds it stated that a quick change of temperature produced a certain reaction which a slow change of temperature failed to evoke. Usually all the phenomena are treated in terms of stimulation, and the absence of reaction with slow change of temperature is regarded as secondary. Were it not for the specific stimulatory effects of quick change, which are not difficult to comprehend as a phenomenon *sui generis*, I hardly think so general a tacit acquiescence would have been extended by botanists to the view that all enduring changes of velocity of metabolism brought about by lasting changes of temperature are stimulatory in nature.

No determination of the rate of development of bacteria through a very wide range of temperature seems to have been made. There are various incidental experiments which indicate values about 2 for the coefficient of increase of metabolism for a rise of 10° C.

CONCLUSION

In this attempt to assert the inevitableness of the action of physical-chemical principles in the cell, I have not ventured upon even the rudiments of mathematical form, which would be required for a more precise inquiry. Bio-chemistry is indeed becoming added to the ever-increasing number of branches of knowledge of which Lord Bacon wrote:

> Many parts of nature can neither be invented with sufficient subtility, nor demonstrated with sufficient perspicuity, nor accommodated unto use with sufficient dexterity, without the aid and intervening of the mathematics.

To me it seems impossible to avoid regarding the fundamental processes of anabolism, katabolism, and growth as slow chemical reactions catalytically accelerated by protoplasm and inevitably accelerated by temperature. This soon follows if we once admit that the atoms and molecules

concerned possess the same essential properties during their brief sojourn in the living nexus as they do before and after.

In his address to the geological section, Professor John Joly, F.R.S., dealt with the effects of the presence of radium in the earth's crust and rocks on the distribution of the temperature gradients. By exhaustive determinations of the radium contents of various rocks and oceanic sediments, as well as by a systematic examination of the rocks of the Simplon and Central St. Gothard tunnels he found the change in temperature gradient observed to correspond exactly with the radium content of the rock. Space will not permit a proper abstract of this interesting address.

Leo Frank Guttman

THE OFFICIAL INSPECTION OF COMMODITIES

The adulteration of articles of commerce is distinctly an evil of civilization. In the primitive state of man, nature supplied directly to the consumer the materials for food and raiment. There was no commerce and, therefore, none of the attending frauds. The savage vented his evil nature in murder, rapine and other of the grosser forms of crime, but he had no opportunity to practise the more intellectual frauds which civilization has made possible.

As soon as commerce came into existence, merchants began to cheat in weight and measure and to practise other commercial frauds, but at first they had comparatively few opportunities for adulteration, as the articles exchanged were for the most part crude products, such as grain and wool, which could not be successfully imitated. It was at a later period, when flour, cloth and other adulterable articles were bought and sold, that sophistication began to be a serious menace to the public welfare, and the evil, having once gained a foothold, increased as civilization advanced.

In recent times adulteration has increased enormously, particularly during the past half century.

There are several reasons for this alarming growth of fraud. In the first place, the number of commercial articles which can be successfully imitated greatly increased during the past century and is still increasing. We have to-day on the market an endless variety of foods, drugs, paints, oils, chemicals and fabrics which can be readily debased by the addition of foreign materials, without having the fraud evident to the purchaser.

The second reason is that the manufacture of butter, lard, cheese, starch, yarn, cloth and other articles, which formerly was carried on in the household, has been transferred to the mill and the factory. It can not be disputed that the cost of production has been reduced by this centralization of labor, and the housewife, incidentally, has been saved a deal of hard work, but the genuineness of the products has suffered as a consequence.

Still another cause for the increase of sophistication in recent years is to be found in the variety of materials adapted for use as adulterants which are now obtainable. Some of the materials which are commonly used for fraudulent purposes are products of the highest scientific research. I will mention as examples—oleo oil, cotton-seed oil, stearine and petroleum products, used for mixing with higher-priced fats and oils; glucose syrup, the common adulterant for molasses; artificial vanillin and coumarin, used in vanilla extracts; salicylic acid, benzoic acid and other food preservatives; coal-tar dyes, which serve as a mask for other food adulterants; wood alcohol, acetanilid and other coal-tar products, also morphine, cocaine and other habit-forming

drugs, used in illegal medicinal preparations; artificial silk, often substituted for true silk, and so on. These products properly used are of great benefit to mankind, but altogether too often they have been parties to frauds and have thereby gained bad repute.

The extent to which adulteration is practised to-day is certainly cause for alarm, although too often the matter is overlooked or considered only from the humorous standpoint. The man who milks the cow with the iron tail, like the boy who steals watermelons, is looked upon as having perpetrated a good joke on the community, when in reality he is committing a criminal offense. All those who manufacture or knowingly sell adulterated products should be regarded as a menace to the welfare of the community, whether or not they are so declared by the laws of the land in which they live. If the article is merely fraudulent and causes no injury to health, the offender belongs in the same class with those who cheat in weight and measure and with common thieves; if, however, it is a menace to the safety or health of the community, he is several degrees lower in the moral scale.

The evil is not one which corrects itself, but, like other crimes, calls for vigilance on the part of both the individual and the state. The consumer should learn to distinguish, as far as possible, the pure from the false, and the state should enact and properly enforce laws for the further protection of the consumer. The necessity of legislative measures has been generally recognized and, as a consequence, inspection laws have been enacted in nearly every civilized country.

In the United States the inspection of milk, in a primitive way, was carried on in some of the large cities as early as the middle of the last century, and perhaps earlier; but it was not until a generation later that milk inspection was placed on a sound chemical basis, and not until still more recently that it has been carried out with the cooperation of the bacteriologist.

The first really extensive movement to prevent adulteration by official inspection and analytical control was the agitation among agriculturists which led to the establishment of experiment stations and the enactment of state fertilizer laws. On this occasion it is of special interest to recall that the first experiment station was established in Connecticut in 1875, largely through the efforts of Professor S. W. Johnson, of the Sheffield Scientific School of Yale University; and after two years' probationary existence at Wesleyan University, was permanently located in New Haven.

Following the example of Connecticut, other states, one by one, established experiment stations and enacted suitable fertilizer laws, until the movement had extended to all the states using commercial fertilizers in considerable quantities.

The official inspection work of the states for some years after the establishment of experiment stations was largely confined to plant foods, disregarding foods for man and the lower animals. It is hard to find an adequate explanation for this early solicitude for the rights of the vegetable kingdom, and the years of almost complete neglect of the dietary grievances of man and beast. Certain it is that the adulteration of foods for animate beings was quite as general as that of fertilizers, and the welfare of the community suffered to a much more alarming extent thereby; however, it is gratifying to note that this inconsistency in the laws has been remedied by more recent measures.

Massachusetts in 1883 and Ohio in 1886 enacted comprehensive food and drug laws,

and, one by one, other states have fallen in line as "Pure Food States." Most of the food laws first enacted affected only dairy products, and their passage was secured chiefly through the efforts of dairymen to protect their interests; later, these laws were amended, or new laws were enacted, so as to include all articles of food and drink, and with the special view of protecting the consumer as well as the honest producer.

The enforcement of food laws, in most states, devolves on a Food Commission, but in a number of states it is in the hands of the Experiment Station or the Board of Health.

The passage of the National Food and Drugs Act in 1906 marked an important era in legislative measures for the protection of the public against fraudulent foods, beverages and medicinal preparations. This law not only renders more effective the state laws by checking the interstate shipment of fraudulent products, but also is proving a stimulus to the enactment of the laws in states previously without food inspection, and an important influence in bringing about uniformity in state laws.

In certain states the inspection of commodities has been carried a step farther. For example, the Maine Experiment Station inspects field and garden seeds with reference to their purity and vitality, and the stations in several other states exercise a semi-official control over the insecticides and fungicides on the market. In North Dakota a paint law is doing much to prevent such gross frauds as the substitution of fish oil and mineral oils for linseed oil, of whiting, barites and other cheap mineral powders for white lead and zinc white, etc. Ever since the discovery of petroleum, efforts have been made to protect the public from illuminating and fuel oils with low flash points and to-day more or less effective laws providing for the inspection of these products are in force in most of the states. Laws against substituting inferior alloys for standard alloys of gold and silver have also been enacted in some states, although, so far as I know, no official inspection of precious metals is in force.

From the foregoing brief statement of the growth of inspection in this country, it is apparent that the sentiment in its favor is wide-spread and the classes of commodities concerned are on the increase. Under the conditions of trade that now prevail inspection is almost a necessity and there appears to be good reason for making the official control of the commodities already named universal throughout the country and extending the system so as to cover still other commodities.

If petroleum is inspected, why not coal; if paints and oils, why not lime and cement; if foods, why not raiment?

The fact that all coal sold for the same grade is not of equal value is well known to those chemists who have studied coal analyses, and also to consumers who have observed the occurrence of slate in the coal itself and clinkers in the ashes. The government inspects and analyzes the coal it buys and it seems not unreasonable that the consumer should also be protected. Such an inspection of coal, if feasible, would be a matter of public economy, not, as in the case of illuminating oils, of public safety.

Building frauds concern both the safety and the pecuniary welfare of the community. It frequently happens that the cupidity or ignorance of the builder leads him to use poor lime and cement, or mix an undue amount of sand with his mortar, thus weakening his structures and imperiling life. The rigid inspection of buildings in the cities does much to protect the public from these dangers, but this inspection is

local and seldom as comprehensive as could be desired. The analytical control of certain building materials may not be impracticable.

The last class of commodities which we will consider is one which, next to foods and drugs, is perhaps most in need of inspection. I refer to textiles and other materials for wearing apparel.

The frauds in woolens have been notorious. Sixty years ago when my father started in business as a country merchant, he learned the value of the match test for cotton, and throughout his life whenever purchasing woolens for personal use he never failed to draw out threads and apply this test, often to the chagrin of the seller. Twenty years ago in this very city I purchased a suit of clothes of splendid appearance at a price which was almost, if not quite, beyond my means. In a few weeks the so-called "all wool" material of the suit showed the characteristic faded appearance of cotton, and was hardly fit for the garb of even a struggling chemist. During the past year one of my friends made a similar purchase in the heart of the shopping district of Chicago. At his request I examined the goods and reported that the warp was all cotton and the woof a mixture of cotton and shoddy. Needless to say there was no redress for this gross imposition.

Frauds in other textiles are not uncommon. So-called linen often contains cotton, hemp and tow; "all silk" often has a cotton back, and much that appears like silk is so-called artificial silk or even mercerized cotton.

There are also glaring frauds in shoes, gloves and other leather goods, felts and furs. Split leather is sold for calf skin, lamb and other inferior leather for kid, wool felt for fur felt, imitation furs for the genuine, and so on.

The more the subject is investigated, the more wide-spread and ingenious appear to be the frauds practised.

Opponents of paternalism will doubtless find cause for alarm in the mere suggestion of the further extension of inspection. They will feel that the rights of the merchant class are threatened and that the dangers from indiscreet and corrupt officials are greater than those from adulteration.

It can not be denied that there are difficulties in the way, and great care should be exercised in framing new laws, but if the present inspection of commodities is, on the whole, an advantage to the public, it seems but logical to extend the system so as to cover other necessities of life liable to adulteration. The inspection should be primarily in the interest of the consumer.

The manufacturer and dealer can usually take care of themselves, as their knowledge of the trade enables them to buy wisely, and in case of doubt submit samples to the chemist for analysis. For years sugar refiners, packing houses, iron and steel works, fertilizer manufacturers, cement works and other manufacturing industries have maintained laboratories for the examination of their raw material, and have proved themselves quite able to look after their own interests.

With the consumer the case is different. He is usually ignorant of trade practises and of the steps necessary to secure chemical evidence of adulteration and redress in the courts. Even if he can afford to seek the advice of the chemist and the lawyer, he is at a disadvantage in fighting large houses with their array of professional advisers. He needs the protection of a system of legal inspection which follows up and punishes a fraud in a five-cent purchase with as much care as a thousand-dollar swindle.

Incidentally, the honest producer is the

gainer by inspection, as the prevention of dishonest competition is distinctly to his advantage. In the past reputable business houses have been among the first to favor adulteration laws, and it is not improbable that they would welcome further measures. It should be remembered, however, that the trade is ever jealous of its rights and is quick to resent any unnecessary interference.

The chemist has been an important factor in inspection, as on him has devolved the important task of securing evidence as to the character or purity of products and presenting this evidence either in official reports or by testimony in court.

The Association of Official Agricultural Chemists, since its inception, has been preeminently a body of inspection chemists and doubtless would never have existed had it not been for laws affecting the sale of fertilizers and foods. The same may be said of the Section of Agricultural and Food Chemistry of this Association. Of the 28 papers to be read at this meeting before the section, at least 19 relate directly or indirectly to inspection, and a great majority of the listeners will doubtless be inspection chemists.

The extension of inspection is continually opening up new and fascinating realms of investigation, and it will be a long time before the analyst need cry for new worlds to conquer.

A. L. Winton

Chicago Laboratory of
Bureau of Chemistry,
U. S. Department of Agriculture

THE AMERICAN SOCIOLOGICAL SOCIETY

The third annual meeting of the American Sociological Society will be held at Atlantic City, N. J., from December 28 to 31, inclusive, in conjunction with the annual meetings of the American Economic Association, the American Statistical Association and the American Association for Labor Legislation.

The Sociological Society will hold seven sessions, one of which will be a joint meeting with the Economic Association and another of which will be a joint meeting with the American Statistical Association. All seven sessions, however, will be devoted to a discussion of some aspect of the general topic "The Family in Modern Society." This general topic has been divided as follows:

1. Relation of the family to social change. (President's address.)
2. How do home conditions react upon the family?
3. Are modern industry and city life unfavorable to the family?
4. How does the woman movement react upon the family?
5. Has the freer granting of divorce proved an evil?
6. How far should the state go in individualizing the members of the family?
7. How far should family property be conserved and encouraged?

Detailed programs can be had by applying to the Secretary, Professor C. W. A. Veditz, George Washington University, Washington, D. C.

THE "UNIVERSITY TABLE" AT THE NAPLES ZOOLOGICAL STATION

For a number of years in the past a table was maintained at the Naples laboratory under the above title, half the cost being met by the late William E. Dodge in the name of Columbia University, and half by contributions from other sources. Owing to lack of regular support the subscription for this table unfortunately lapsed for several years. Friends of the university have now made it possible to renew the subscription, and it is hoped that the table may now be permanently maintained under the name of the "Columbia Table." Its use is open to all qualified Americans and applications may be sent to Professor E. B. Wilson, Columbia University, New York City.

SCIENTIFIC NOTES AND NEWS

The Nichols Medal of the American Chemical Society has been awarded to Professor William A. Noyes, of the University of

Illinois, and Dr. H. C. P. Weber, for their researches on the atomic weight of chlorine. The medals will be awarded at a meeting of the Chemists' Club, New York City, on the evening of November 26, when Dr. Noyes will give a very brief résumé of the work on chlorine, and Dr. Weber will announce further results on work which has been done along the same line on the atomic weight of bromine.

THE Bisset Hawkins medal, of the Royal College of Physicians, of London, awarded triennially to a British medical man who had distinguished himself in sanitary science and the promotion of public health, has been presented to Sir Shirley Murphy, medical officer to the London County Council, in recognition of his services to the public and to preventive medicine.

AT the annual meeting of the American Mathematical Society, to be held in the last week of December, President H. S. White will deliver his retiring address, the subject of which will be "Bezout's theory of resultants and its influence on geometry."

IN the vote for the Lord Rectorship of Edinburgh University, Mr. George Wyndham received 826 votes, Mr. Winston Churchill 727 votes and Dr. William Osler 614 votes.

PROFESSOR BASHFORD DEAN will represent Columbia University at the inauguration of the monument to Lamarck, in Paris, on November 19.

PROFESSOR CHARLES R. BARNES and Dr. W. J. G. Land, of the University of Chicago, are spending four months (September–December) in tropical Mexico, primarily to investigate the Bryophytes, and incidentally to secure other research material for the department of botany.

THE Geological Survey has issued a report on the prevention of mine explosions, submitted by three foreign experts, Victor Watteyne, inspector-general of mines, Belgium; Carl Meissner, councilor for mines, Germany, and Arthur Desborough, H. M. inspector of explosives, England. These engineers have been in the United States for six weeks, coming at the invitation of the government to assist the federal authorities in beginning the investigations authorized at the last session of congress.

MR. HENRY LEIGHTON, instructor in geology, Cornell University, has resigned to take a position as assistant in economic geology in the New York State Museum, Albany.

MR. V. BLEININGER, of Champaign, Ill., has been appointed ceramic chemist of a new section of the U. S. Geological Survey devoted to clays and clay products.

PROFESSOR JOHN M. MACFARLANE, of the University of Pennsylvania, is chairman of a committee of the Bartram Association, which will plant a tree annually in Bartram's Gardens, either at the June or October meeting of the association. At a meeting of the society held on Saturday, October 24, Henry R. Edmunds, president of the board of education, was elected president.

MR. H. L. TELEGDI is at present visiting this country in the interests of the agricultural department of the Hungarian government.

G. SUTO, professor of veterinary science in the Agricultural College at Tokyo, has passed through the United States on his way home, after a visit to Germany.

D. SHONO and Professor Gamoh, of Tokyo, are in this country, having been appointed by the emperor of Japan to investigate industrial conditions in America, particularly in connection with industrial education in our secondary schools and colleges.

DR. JOSEPH S. CHAMBERLAIN, chief of the cattle food and grain investigation laboratory of the Bureau of Chemistry, U. S. Department of Agriculture, has been granted leave of absence for a year's study abroad. He expects to work under Professors Fischer and Abderhalden at the University of Berlin.

DR. NICOLAS ACHÚCARRO, physician to the General Hospital at Madrid, Spain, has been appointed and has taken up his duties as histopathologist at the Government Hospital for the Insane, Washington, D. C. Dr. Achúcarro was granted a year's leave of absence by the Spanish government for this purpose.

CAPTAIN C. C. CARTER, U. S. M. A., 1899, has been detailed by the War Department to spend

the current year at the Massachusetts Institute of Technology studying electrical engineering. Captain Carter is attached to the Coast Artillery Corps, and for the past few years has been an instructor in the Fortress Monroe Artillery School and the Fort Totten School of Submarine Defence.

The inaugural lecture of Professor Albrecht Penck, of the University of Berlin, as Kaiser Wilhelm professor at Columbia University, was given on November 4, the subject being "The Face of the Earth."

Professor William James will repeat the series of eight lectures, which he gave recently at Oxford University, in Emerson Hall. The course is entitled "The Present Situation in Philosophy."

At the Dublin meeting of the British Association for the Advancement of Science, September 2–9, Professor A. Lawrence Rotch discussed, before the Physical Section, the warm stratum in the upper air. At the jubilee meeting of the German Meteorological Society, which was held at Hamburg, September 28–30, Professor Rotch read a paper entitled "Die warme Schicht der Atmosphäre oberhalb 12 Km. in Amerika."

The lectures on certain fundamental problems in physiology common to animals and plants to be given by Dr. W. M. Bayliss, F.R.S., at University College, London, will be devoted to the permeability of cells and membranes and the phenomena connected therewith, such as plasmolysis, secretion, nature of the nerve impulse, etc. The lectures, which began on October 21, are given on Wednesdays at 5 P.M.

Dean Leonard Pearson, of the veterinary medical department of the University of Pennsylvania, delivered an address at the milk and dairy exhibit in the Chamber of Commerce at Pittsburg, Pa., under the auspices of the United States Department of Agriculture on October 22.

The three hundredth anniversary of the election of Francis Bacon as treasurer of Gray's Inn, which took place on October 17, 1608, was celebrated October 17 by a luncheon at the inn, at which the benchers entertained a number of distinguished guests. It is the intention of members of the inn to observe the first night of term (November 2) as a Bacon anniversary, and, at a later date, a permanent memorial will be placed in one of the open spaces of the inn—probably south-square. This memorial will consist of a marble statue of Bacon by Mr. F. W. Pomeroy, A.R.A. A sketch model of it was on view October 17, as well as a collection of Baconian manuscripts and printed books.

A bronze tablet to the memory of the late Major James Carroll will be unveiled in the main medical building of the University of Maryland, on November 11. Dr. William H. Welch will deliver the principal address.

F. A. C. Perrine, A.B. (Princeton '83) and D.Sc. ('85), from 1893 to 1900 professor of electrical engineering at Stanford University, and since then a consulting engineer, died at Plainfield, N. J., on October 20, aged forty-six years.

Professor Berger, the eminent French surgeon, who was seized with an attack of apoplexy while about to perform an operation at the Neckar Hospital, on October 10, died without recovering consciousness. He was born in 1845.

There will be a civil service examination on November 16 and 17 for the position of assistant (male) in the U. S. Naval Observatory at a salary of $1,000.

The National Association of State Universities will meet in Washington, D. C., on November 16 and 17.

The eighth annual meeting of the American Philosophical Association will be held at Baltimore, on Tuesday, Wednesday and Thursday, December 29, 30 and 31, 1908.

The second regular meeting of the southwestern section of the American Mathematical Society will be held at the University of Kansas, Lawrence, Kansas, on Saturday, November 28.

The twenty-sixth stated meeting of the American Ornithologists' Union will be held in Cambridge, Mass., beginning on the evening of November 16, at 8 o'clock. The evening

session will be for the election of officers and members, and for the transaction of other routine business. The meetings, open to the public and devoted to the presentation and discussion of scientific papers, will be held at the University Museum, Oxford Street, commencing on Tuesday, November 17, and continuing for three days. Information regarding the meeting can be had by addressing the secretary, Mr. John H. Sage, Portland, Conn.

THE Salters' Company, London, has voted £100 a year for a period of three years to the cancer research laboratories of the Middlesex Hospital as a research scholarship.

LIEUTENANT-COLONEL BURLAND, of Montreal, has given $50,000 to the Montreal League for the Prevention and Treatment of Tuberculosis. In the heart of Montreal, Colonel Burland has presented a building for dispensary purposes and the gift of $50,000 is on the condition that the league raises an endowment of a similar amount.

A ROYAL COMMISSION has been appointed in Great Britain to make an inventory of the ancient and historical monuments and constructions connected with or illustrative of the contemporary culture, civilization and conditions of life of the people in England from the earliest times to the year 1700, and to specify those which seem most worthy of preservation. The commission is constituted as follows: Lord Burghclere (chairman); Earl of Plymouth, C.B.; Viscount Dillon; Lord Balcarres, M.P.; Sir H. H. Howorth, K.C.I.E., F.R.S.; Sir John F. F. Horner, K.C.V.O.; Mr. E. J. Horniman, M.P.; Dr. F. J. Haverfield, Camden professor of ancient history in the University of Oxford; Mr. L. Stokes, vice-president of the Royal Institute of British Architects; Mr. J. Fitzgerald, assistant secretary to H.M. Office of Works; and Mr. J. G. N. Clift, hon. secretary to the British Archeological Association.

PROFESSOR SENECA EGBERT will give a series of free public lectures at the Academy of Natural Sciences, Philadelphia, on "The Prevention of Disease and the Preservation of Health," at 8 P.M. on the following dates:

November 5—"The Economic Loss due to Preventable Diseases—What they cost and how they Affect the People, Individually and Collectively."

November 12—"What is being done to prevent Disease—Results already Achieved—The Outlook for the Future."

November 19—"Methods of Prophylaxis, and their Efficiency—The Value of Sanitation, Quarantine, Vaccination, etc."

November 27—"The Importance of Personal Health in Preventing Disease—Necessity of Fostering and Increasing One's Vital Resistance—The Influence of Extraneous Factors as Pure Air, Pure Water, Pure Food, etc."

December 3—"The Importance and Need of Popular Education in Public Health—Available and Practical Means for Securing It—Missionary Work Imperative for the Immediate Future."

THE following is a series of lectures which has been arranged by the Museum of the University of Pennsylvania, for the month of November. They are to be given in the Widener Lecture Hall, at three o'clock on the afternoons mentioned.

November 7—C. Leonard Woolley, A.M.: "Results of the Second Eckley B. Coxe Egyptian Expedition" and "Excavations in Nubia."

November 11—Sir Harry Johnston, G.C.M.G., K.C.B., D.Sc.: "The Congo State—Its Fauna and Its Peoples."

November 18—Professor Oscar Gustaf Monetilius, Royal Antiquary of Sweden: "The Mycenean Period."

November 25—C. Leonard Woolley, A.M.: "A Roman Town in Britain" and "Roman Britain in the Light of the Present Excavations at Corbridge on Tyne."

THE ADMINISTRATION OF THE UNIVERSITY OF ILLINOIS

AT a meeting of the senate of the University of Illinois on October 15, called at the request of the committee on educational policy, the following resolutions were adopted:

WHEREAS, There is ground for apprehending that recent articles in the press may lead the public to think that academic freedom is suppressed or interfered with at the University of

Illinois by the president, or that tenure of office is insecure because of autocratic administration; therefore, without entering at all into a discussion of the case referred to in said articles, be it

Resolved, by the senate of the University of Illinois (a body which includes all heads of departments and full professors in the university), that it is our belief that each member of the faculty has entire freedom of opinion, that he is free to express his opinions on all matters of university administration and educational policy to his colleagues and to the president without interference and without fear that it will endanger his position.

Resolved, That we hereby express our confidence in the president of the university and our conviction that he administers his high office as a colleague rather than as a superior.

Resolved, That in the opinion of the university senate the course of the administration has been such as to stimulate to a marked degree the higher scientific and educational interests of the university.

Resolved, That as members of the faculty we assure the president of our loyal and hearty support in the varied and difficult responsibilities imposed upon him as the executive head of this university.

UNIVERSITY AND EDUCATIONAL NEWS

The Draper's Company will erect for Oxford University an electrical laboratory at a cost of £22,000 and will give an additional sum of £1,000 for its equipment.

The H. K. Cushing Laboratory of Experimental Medicine at Western Reserve University will be dedicated on Friday, November 20, at 2 o'clock, when Professor William H. Welch, of the Johns Hopkins University, will make the address.

Cornell University has bought within the past year for the benefit of the College of Agriculture and the Veterinary College approximately 500 acres of ground lying contiguous to its other holdings.

Mrs. Mary Fiske Spencer, of Oberlin, has presented to the botanical laboratory at Oberlin College a collection of seven thousand European plants, gathered during twenty-five years of residence in Munich.

The council of the Royal College of Surgeons, of London, has adopted resolutions which will in future admit women to the examinations of the conjoint examining board in England, to the examination for the diploma in public health, to the examinations for the fellowship, and to the examinations for the license in dental surgery.

Dr. Florian Cajori, head professor of mathematics in Colorado College, has again accepted the deanship of the Engineering School.

Dr. Daniel Starch, instructor in experimental psychology in Wellesley College, has been appointed instructor in psychology in the University of Wisconsin.

Dr. T. H. McHalton has resigned as horticulturist of the Georgia Experiment Station, to take the position of adjunct professor in charge of the Horticultural Department of the Georgia State College of Agriculture at the University of Georgia, Athens.

DISCUSSION AND CORRESPONDENCE

DR. O. P. HAY ON THE SKULL OF DIPLODOCUS

My attention has been called to-day to an article in Science from the pen of my friend, Dr. O. P. Hay, in which he indulges in certain criticisms of my paper on the "Osteology of Diplodocus" published in the *Memoirs* of the Carnegie Museum, Volume II., page 225 *et seq.*

"*Humanum est errare*," and it is quite possible I have made mistakes. I shall be glad to accept corrections when I am convinced they are well founded. Until, however, I have time to reexamine the whole subject I am not inclined to adopt Dr. Hay's opinion as final. His *ipse dixit* does not carry conviction with it, especially as I am aware that he is only beginning his studies in this difficult field of investigation. We all acknowledge him to be a competent student of the Testudinata, but his investigations as to the skulls of the dinosauria are of quite recent origin.

Leaving out of sight difficult questions which relate to the interpretation of the foramina of the skull intended to give exit to

the nerves issuing from the brain, which require careful reexamination at leisure, and passing over as not worthy of comment the *lapsus calami* on page 235 to which he calls attention, and the importance of which he magnifies, as it is abundantly corrected elsewhere, I wish to protest against the misrepresentations contained in his article, founded upon a total failure to understand my meaning and attitude.

Dr. Hay labors to make it appear that I suggest that *Diplodocus* was an animal "with three pairs of nostrils." I never suggested such a thought and no fair interpretation of my words could lead to it. In my paper I simply called attention to the obvious fact that two openings in the bones of the skull apparently leading into the narial cavity occur on either side in advance of the large posterior opening, which Professor Marsh interpreted as the nasal aperture. *They are there!* Dr. Hay is at liberty to amuse himself at his leisure in endeavoring to explain them as he pleases. I did say of the foramina, which I named "the mesial foramina of the maxilla," that they might possibly have had "a function supplementary to the function of the true narial opening." This does not necessarily imply that they were nostrils opening from the nasal cavity into the outer air. Whether they were nares, or were covered by tegument in life, I did not venture to say. It is quite conceivable that the large opening which Professor Marsh has interpreted as the true narial opening may have been covered by tegument and that one or the other of the smaller pairs of anterior openings may have been the true functional nares, as was long ago suggested, if I remember correctly, by the late Dr. Baur. In attempting to make me to have suggested that *Diplodocus* had three pairs of functional nares Dr. Hay is traveling quite beyond my text, and this "fanciful proposition," as he is pleased to call it, is the creature of his own imagination. I object to have him thus misinterpret me.

Dr. Hay devotes a paragraph to a foot-note on page 245 which he does not quote, but which he garbles. This foot-note is as follows: "*Sphenodon* has no external ear, agreeing in this respect with many other recent reptilia and ophidia. It is possible that *Diplodocus* had no external ear." I might have omitted the word "many," and have written the word "probable" instead of the word "possible," but then I do not claim omniscience. Omniscience is not one of my fads; and besides I know, as Dr. Hay should also know, that we have in some of the batrachia and lacertilia folds of skin partially covering the tympanum, suggesting and apparently to a certain extent functioning as rudimentary outer ears, and that in the crocodilia there is provided an opercular flap, which distinctly functions as an outer ear. I think my temperate statement may stand as I left it. It does not imply, as Dr. Hay tries to twist it into implying, that I held the ridiculous opinion that there exist reptilia with outer ears fully developed, as for instance, in the mammalia. Dr. Hay in his article is evidently making an attempt to be "funny." He ought first to be sure that he understands what he is talking about.

W. J. HOLLAND

CARNEGIE MUSEUM,
PITTSBURG, PA.,
October 20, 1908

ON THE ENCOURAGEMENT OF MR. CHARLES D. SNYDER

IN a recent paper[1] Charles D. Snyder has published the following statement:

> If we believe that any given physiological activity is due to some particular physical change, we need only to determine at which velocities the action proceeds under various temperatures and then compare these results with the velocities of (probable) physical processes under similar changes of temperature in order to test for ourselves the correctness of our view.

He here refers to a foot-note which reads as follows:

> See the author's *original* communication, *Archiv für Anatomie und Physiologie, Physiol. Abh.*, 1907, p. 113. In this paper the idea of comparing temperature coefficients for possible physical causes underlying physiological actions, as outlined above, was clearly expressed. It was

[1] Snyder, Chas. D., *Am. Jour. Physiol.*, Vol. 22, No. 3, p. 309, August, 1908.

clearly stated in the abstract of the present paper, as published first at the Congress in Heidelberg, August, 1907, and later in the proceedings of the same which appeared in the various journals and archives of physiology during the fall of the same year. Since that time it is encouraging to note that J. Loeb (*Journal of Biological Chemistry*, October, 1907) and J. Bernstein (*Pflüger's Archiv*, 1908, CXXII., p. 129) have both thought well enough of the idea to use it as a basis for investigations in their own laboratories.

The italics in the above quotation are inserted by the writers of this note, since the word italicized renders the sentence in which it occurs a deliberate misstatement. Mr. Snyder's original communication, to which, through some curious oversight, he omits to refer either in this paper or in his paper in the *Zentralblatt für Physiologie* (22: 1908, s236), appeared in the University of California publications in 1905.

We think it advisable to draw attention to the fact, doubtless forgotten by Mr. Snyder in the stress of scientific production, because the statements quoted above would lead the reader, unacquainted with the history of this phase of physiological investigation, to suppose that members of this laboratory had utilized Mr. Snyder's results and views without giving him the full credit which he deserves. The true situation is, however, exactly the reverse, and it is for the purpose of removing the stigma which has thus been placed by Mr. Snyder upon the workers in this laboratory, that this short note is written.

It can not fail to strike an observant reader of Mr. Snyder's paper as very curious that he does not in his "conspectus of the temperature coefficients of the velocities of all the physiological actions determined up to the present time" refer to the paper by S. S. Maxwell entitled "Is the Conduction of the Nerve Impulse a Chemical or a Physical Process?" published in the *Journal of Biological Chemistry*, October, 1907, and this fact will appear the more strange when the reader observes that in the foot-note quoted above, on page 310 of Snyder's paper, this very publication is alluded to as one of the investigations from the laboratory of J. Loeb, which "it is encouraging to note" has been based upon the ideas of Mr. Snyder. From the wording of this footnote it would appear that Maxwell overlooked Snyder's publications and, in particular, omitted to refer to his publication upon the transmission of the nerve impulse published in April, 1907. So far is this from being the case that a considerable proportion of Maxwell's publication was devoted to a criticism of this publication of Snyder's. The most damaging criticism was, however, omitted from Maxwell's paper out of regard for Mr. Snyder. Should Mr. Snyder desire it, however, that criticism can still be published.

The actual succession of events which led to the appearance of this series of publications was, as far as this laboratory is concerned, as follows:

In 1903 Martin H. Fischer, at that time a member of this department, translated Cohen's "Physical Chemistry." In the fourth chapter of this work Cohen reviewed the work of Arrhenius and van't Hoff upon the temperature coefficients of chemical reactions, and showed that the temperature coefficient of various life-processes was that of a chemical reaction-velocity. He also suggested that the same principle might be found to apply in many other physiological processes.

The ideas thus expressed by Cohen led Loeb to consider it advisable that a series of investigations upon the temperature-coefficients of life-processes should be undertaken, with the view of ascertaining whether these processes are determined primarily by physical or chemical agencies.[2] For this reason he assigned to Mr. Snyder, at that time a candidate for the degree of Ph.D., the problem of ascertaining the temperature-coefficient of the heart-beat, and urged him to present his results as his thesis for the degree. This origin of his investigation is very clearly stated by Mr. Snyder in his own words, as follows:[3]

In his book on "Brain Physiology," Loeb mentioned the possibility that the heart beat might be caused by a fermentative process which is going

[2] Cf. Loeb, *Univ. of Calif. Publ., Physiol.*, 3, 1905, p. 3.

[3] *Univ. of Calif. Publ., Physiol.*, 2, 1905, p. 126.

on constantly in the heart. He suggested to me that this idea could be tested by exact quantitative determinations of the influence of temperature upon the rate of the heart beat. We know through Arrhenius how the velocity of a chemical reaction varies with the temperature. If the heart beat were, as supposed, a function of a reaction velocity, then the law of Arrhenius should also hold for the influence of temperature upon the rate of the heart beat.

While Mr. Snyder was preparing his thesis for the degree of Ph.D., Loeb suggested to Maxwell that he should undertake the investigation of the influence of temperature upon the velocity of the nervous impulse: Maxwell, however, started work upon another problem, and consequently Loeb asked Burnett to undertake this investigation. Burnett's investigation had not proceeded far when he found it advantageous to study the influence of temperature upon the latent period of striated muscle. The result of this investigation was published early in 1906.[4] Afterwards, on account of pressure of other work, Burnett was unable to continue the investigation, and, at Loeb's request, Maxwell carried it through, and the results were embodied in the paper to which Mr. Snyder refers, in the foot-note quoted above, as an encouraging example of the extent to which his ideas have been appropriated by members of this laboratory. Meanwhile Loeb had published his observations upon the temperature coefficient of artificial maturation in *Lottia*,[5] and of the production of artificial parthenogenesis by the action of hypertonic sea water,[6] and Robertson had published his observations upon the temperature coefficient of the heart beat in the crustacean *Daphnia*.[7] In his "conspectus of the temperature coefficients of the velocities of all the physiological actions determined up to the present time," however, Mr. Snyder does not refer to any of these papers, and, with remarkable consistency, also fails to refer to all papers upon the temperature coefficients of life processes which contain extensive references to these publications, as, for example, the paper on the influence of temperature upon the refractory period in the sartorius muscle of the frog, published by Bazett in the *Journal of Physiology*, February, 1908. Mr. Snyder is, however, undoubtedly familiar with some of these publications, inasmuch as he has inserted references to that of Loeb upon the temperature coefficient of artificial maturation and to that of Robertson upon the temperature coefficient of the heart beat in *Daphnia*, in a previous publication of his own.[8]

Since Mr. Snyder's "conspectus of the temperature coefficients of the velocities of all the physiological actions determined up to the present time" is singularly defective in other respects, we here insert a list of the literature on this subject which, to the best of our knowledge is complete up to the present date. Save those containing results originally utilized by Cohen in calculating the temperature coefficients of life processes published in his "Physical Chemistry," all references to papers not actually regarding the influence of temperature upon life phenomena from the point of view of its influence upon chemical reaction velocity, are omitted. We publish this list in the hope, not only that it may be of use to the student of this department of physiological research, but also that it may serve to further encourage Mr. Snyder by demonstrating to him how widely his ideas have been adopted, how many laboratories have thought well enough of them to use them as a basis for investigations and in how many cases these ideas were, with remarkable prescience, utilized before Mr. Snyder had ever published his "original communication,"[9] in which they were so "clearly expressed":

Clausen: *Landwirtsch. Jahrb.*, 19, 1890, p. 892.

Hertwig, O.: *Arch. f. Mikrosk. Anat.*, 51, 1898, p. 319.

Cohen, Ernst: "Physical Chemistry," trans. by Fischer, New York, 1903, pp. 50–67.

[4] *Journal of Biological Chemistry*, 2, 1906, p. 195.

[5] *Univ. of Calif. Publ., Physiol.*, 3, 1905, p. 1.

[6] *Loc. cit.*, p. 40.

[7] *Biol. Bulletin*, 10, 1906, p. 242.

[8] *Amer. Jour. Phys.*, 17, 1906, p. 350.

[9] *Arch. für Anat. und Physiol., Physiol. Abh.*, 1907, p. 113.

Aberson: *Rec. d. traver. chim. d. Pays-Bas et de la Belg.*, 22, 1903, p. 78.
Herzog: *Zeit. f. Physiol. Chem.*, 37, 1903, pp. 149 and 396; *Zeit. f. Electrochemie*, 11, 1905, p. 820.
Matthaei, G.: *Phil. Trans. Roy. Soc. London*, 197 B, 1904, p. 47.
Snyder, C. D.: *Univ. of Calif. Publ. Physiol.*, 2, 1905, p. 125; *Am. Jour. Physiol.*, 17, 1906, p. 350; *Arch. f. Physiol.* (Engelmann), 1907, p. 113; 1907, p. 118; *Am. Jour. Physiol.*, 22, 1908, p. 179; 22, 1908, p. 309; *Zentralblatt f. Physiol.*, 22, 1908, p. 236.
Peter, Karl: *Arch. f. Entwicklungsmechanik*, 20, 1905, p. 130.
Abegg: *Zeit. f. Electrochemie*, 11, 1905, pp. 528 and 823.
Euler: *Zeit. f. Physiol. Chem.*, 44, 1905, p. 53.
Senter: *Jour. Physical Chem.*, 9, 1905, p. 311.
Loeb, J.: *Univ. of Calif. Publ., Physiol.*, 3, 1905, p. 1; 3, 1906, p. 39; "Dynamics of Living Matter," New York, 1906, lecture 6; *Pflüger's Arch.*, 124, 1908, p. 411.
Kanitz, A.: *Zeit. f. Electrochemie*, 11, 1905, heft 42; 13, 1907, p. 707; *Pflüger's Arch.*, 118, 1907, p. 604; *Biol. Centralblatt*, 27, 1907, p. 11.
Robertson, T. Brailsford: *Biol. Bull.*, 10, 1906, p. 242; *Arch. Internat. de Physiol.*, 6, 1908, p. 388.
Taylor, A. E.: *Jour. Biol. Chem.*, 2, 1906, p. 87.
Jost: *Biol. Centralblatt*, 26, 1906, p. 225.
Burnett, T. C.: *Jour. Biol. Chem.*, 2, 1906, p. 195.
Madsen and Nyman: *Zeit. f. Hygiene und Infektionskrank.*, 57, 1907, p. 388.
Maxwell, S. S.: *Jour. Biol. Chem.*, 3, 1907, p. 359.
Arrhenius, S.: "Immunochemistry," New York, 1907, chap. 3 and 4.
Bazett, H. C.: *Jour. Physiol.*, 36, 1908, p. 414.
Lucas, Keith: *Jour. Physiol.*, 37, 1908, p. 112.
W. J. Woolley: *Jour. Physiol.*, 37, 1908, p. 122.
Bernstein, J.: *Pflüger's Arch.*, 122, 1908, p. 129.

Jacques Loeb, T. Brailsford Robertson, S. S. Maxwell, Theo. C. Burnett
Rudolph Spreckels Physiological Laboratory, University of California

QUOTATIONS

PUBLICATIONS OF THE WISTAR INSTITUTE

The publication of five important biological journals under the direct control of the Wistar Institute of Anatomy of the University of Pennsylvania has attracted widespread attention among anatomists and zoologists of the country. The step is significant, not so much because the institute has acquired five well-established biological journals, but because it marks an important advance in the cooperation, so much talked about recently, among institutions which consider it their duty to devote some of their best energies to the advancement of human knowledge.

The Wistar Institute began its career as a publishing institution by the distribution of Bulletin No. 1, which was a three-page leaflet setting forth some of the plans the institute proposed to follow in promoting anatomical science. This was in 1905. At the close of 1908 the institute is publishing five journals, with a combined yearly output of about 3,000 pages. Several of these journals are self-supporting, while others incur a considerable yearly deficit. The institute has been able to assume the entire financial responsibility of these publications, without encroaching upon its regular income devoted to its museum and research work, through the efforts and enthusiasm of Dr. Horace Jayne. Doctor Jayne, who is in charge of the newly created department of publication, has done much to improve the journals, increase the subscription lists and has been untiring in his efforts to put the department upon a successful working basis. No similar combination of biological journals has ever been attempted in this country, and the Wistar Institute is to be congratulated upon the success attained in this new venture.

The first journal acquired by the Wistar Institute was the *Journal of Morphology*. Founded in 1887 by Professor C. O. Whitman,

this journal has completed eighteen volumes, which are esteemed both at home and abroad for their valuable scientific contents and the excellence of book-making. In 1903, for lack of funds, it temporarily suspended publication. . . .

Following the lead of the *Journal of Morphology* came the *Journal of Comparative Neurology and Psychology*, the *American Journal of Anatomy*, the *Anatomical Record* and the *Journal of Experimental Zoology*. These journals were assigned to the Wistar Institute because of its efforts to bring about a mutually beneficial cooperation which would lead to greater scientific results with the same outlay of time and money.

They comprise nearly all the independent technical journals in their respective sciences. Their editors are leading men in the branches they represent, and the articles published are the best results from American laboratories.

They represent no school or exclusive band of workers, nor any group of self-centered laboratories. On the contrary, the chiefest aim is to obtain and retain for them an eminently national character, and encourage through the highest grade of biological research the efforts and cooperation of investigators wherever found. . . .

These journals have been and will always be a source of pride to American scholarship, and they are indispensable to those who desire to keep abreast of the times. The effort to increase their circulation is principally to put the original work done in America before as large a number of students as possible, here and abroad—that all may share in this movement for a more vigorous life in these branches.

The chief idea, which the Wistar Institute is following in publishing these journals, is to maintain the editorial management, so far as possible, outside of its own staff. The reasons are evident. The results thus far are most gratifying, and there is every reason to expect a very great increase in the efficiency and value of the undertaking.

The work of this department of the Wistar Institute alone brings the institute and the University of Pennsylvania into relations with nearly every laboratory in the world where anatomy and zoology are studied.—*Old Penn, Weekly Review of the University of Pennsylvania.*

SCIENTIFIC BOOKS

Modern Electrical Theory. By NORMAN ROBERT CAMPBELL, M.A. Pp. xii + 332. Cambridge University Press. 1907.

The past fifteen years have witnessed the erection, upon the foundation of Maxwell's theory, of a great structure of theoretical and experimental knowledge which, for some time to come, will undoubtedly occupy a very large place in the interest and attention of students of physical science. Maxwell's theory was, in the main, the work of a single man of genius; it was general in its point of view, was little concerned with details, and thus possessed a kind of obvious unity which made it easy of comprehension when once the initial difficulties had been overcome. The modern development of electrical theory, on the other hand, has had to deal in great detail with a large number of complex phenomena of apparently diverse character. What is now called the electron theory is the result of the labors of many men, working in different branches of physics and chemistry, with various points of view, and often without recognition of the general, theoretical bearing of their results.

Under these conditions a work like the one before us is of especial value and utility. The author has chosen his material wisely and combined it with skill; he has given a simple and perspicuous account of the theory and of its application to the many and diverse phenomena which have been brought within its scope. The introduction of unnecessary details has been avoided, and although there are inevitably a great many trees, the forest is still distinctly visible. The perspective is thoroughly good and the point of view is not that of the popularizer of second-hand knowledge. Mr. Campbell has worked for many years in the Cavendish Laboratory, which for two generations has been the chief center of progress in electrical science; he has made important contributions to the theory of

which he writes, and his book reveals an intimate and professional knowledge of his subject. It is not, however, addressed exclusively to a professional audience; while not exactly food for babes, it does not assume an extensive preliminary knowledge of the subject, and the necessary mathematical developments are presented in an elementary way, so that any one who is not averse to recalling his knowledge of elementary algebra and geometry may read the book with pleasure and profit.

A marked characteristic of modern physics is the free and fearless use of hypotheses—a use which would have been regarded as dangerous, or at least unscrupulous, in the days before we had been taught by the example of great masters like Faraday, Kelvin and Maxwell, that hypotheses were the most useful of all the instruments of research. In those days a hypothesis was considered to be justifiable only when its author could look forward with confidence to the time when it should be raised to the greater dignity of a "theory," and perhaps ultimately be proved to be "true." How far we have advanced from this position is indicated by Professor J. J. Thomson's remark that a physical theory is not a creed, but a policy, and by Mr. Campbell's statement (p. 231) that "a false hypothesis is better than none." This attitude must, of course, be thoroughly understood by the reader. He must recognize that the model structures which are used in this book to explain electrical and optical phenomena are not the only ones possible even at the present time; and that, as investigation proceeds, they will have to be modified and in many cases rejected altogether in favor of others which more perfectly represent the results of experience.

H. A. Bumstead

Einleitung in die Experimentelle Morphologie der Pflanzen. Von Dr. K. Goebel. 8vo, pp. vi + 260, with 135 figures. Leipzig and Berlin, B. G. Teubner. 1908. Price 8 Marks.

This book, which is an amplification of a series of lectures delivered by Professor Goebel in the winter of 1906–7, is one of the most suggestive botanical contributions of recent years. Not only botanists of moderate training, but scientific gardeners should be able to read the work, repeat the experiments and devise new ones. The apparatus is usually very simple, as the author says, "a plant, a pot of dirt and a question." Little attention is paid to the direct effect of light, heat, etc., the reader being directed to Pfeffer's "Pflanzenphysiologie," and Goebel's "Organographie" for a discussion of these factors. For experimental work on lower plants, reference is made to Klebs's "Ueber Probleme der Entwickelung."

There is no attempt at completeness, the book being intentionally an introduction rather than a hand-book.

The titles of the five chapters indicate the scope of the work. (1) The Field of Experimental Morphology, (2) Influencing the Form of the Leaf by Internal and External Conditions, (3) Conditions for the Various Development of Main and Side Axes, (4) Regeneration, (5) Polarity.

In addition to the question, *How* does development proceed, there is another, *why* does it so proceed. The book is most deeply concerned with the second question. Plants diverging from the usual form are called freaks; some plants develop one form under moist conditions and another under dry; some plants have juvenile stages quite different from the adult form; injuries often cause a plant to develop in a direction not followed by the normal plant, etc. Experimental morphology attempts to answer the questions raised by such phenomena. That heredity must be reckoned with is not questioned. The acorn gives rise to an oak, and the beech nut to a beech tree; but normal stages in development may be skipped, after a later stage there may be a return to an earlier, and this because the various stages in development are dependent upon internal conditions which may be influenced by external factors. Development may be checked at a certain stage, when conditions for the succeeding stage are not present.

The experiments and inferences from them are numerous and suggestive. It may be that strict morphologists lay too much stress upon heredity and try to explain too many phenomena as due to recapitulation. It would seem to the reviewer that Professor Goebel has underestimated the importance of heredity as much as most morphologists exaggerate it.

CHARLES J. CHAMBERLAIN

SCIENTIFIC JOURNALS AND ARTICLES

The American Journal of Science contains the following articles: "Some New Measurements with the Gas Thermometer," by A. L. Day and J. K. Clement; "Range of the α-rays," by W. Duane; "Alteration of Augite-ilmenite Groups in the Cumberland, R. I., Gabbro (Hessose)," by C. H. Warren; "Studies in the Cyperaceæ. XXVI. Remarks on the structure and affinities of some of Dewey's *Carices*," by T. Holm; "Applications of the Lorentz-FitzGerald Hypothesis to Dynamical and Gravitational Problems," by H. A. Bumstead.

SPECIAL ARTICLES

ELECTRIC DISTURBANCES AND PERILS ON MOUNTAIN TOPS

IN view of the scientific interest that has been aroused by the sudden death of mountaineers on the widely separated peaks of San Gorgonio and Whitney during apparently the same electric storm in June, 1904,[1] the following recent experience of Captain R. M. Brambila, U. S. Infantry, and the writer will be welcomed as furnishing some hint of the power and magnitude of such electric disturbances.

This experience was endured by the party during the regular visit to the automatic weather observatory maintained by the Nevada Agricultural Experiment Station on Mount Rose (altitude 10,800 feet), the dominating peak north of Lake Tahoe (on the California-Nevada state line), and approximately 200 miles north of Mount Whitney.

The storm, which was mainly electric in nature, displayed itself first on the evening of Friday, October 19, 1907, in a heavy cloud mass lying close along the crest of the Carson Range north of Mount Rose, but in no wise involving Mount Rose itself. The flashes of lightning were frequent and heavy. Little thunder, if any, however, was heard. On the morning of the twentieth, when the actual ascent of Mount Rose began, clouds gathered from the direction of Lake Tahoe about the summit, and enveloped it somewhat persistently during the day. The wind did not exceed ten miles per hour, and the temperature remained above freezing.

From the summit itself the cañons below could be seen filled with masses of vapor. As night darkened a moderate storm of hail and snow with rain began to fall. The pack horse, which had been stabled on a terrace just below the observatory, was covered from tail to ears to protect him from the pelting missiles.

[1] The distance between these peaks, which lie on opposite sides of the Mojave Desert, southern California, is approximately 180 miles and the difference in elevation is 5,015 feet, the higher peak, Mount Whitney (altitude 14,515 feet), being the highest mountain in the United States, excluding Alaska.

The death on San Gorgonio, said to be the first case of the kind in San Bernardino County, occurred July 24, 1904, that on Mount Whitney two days later, July 26. Referring to these fatalities, Professor Alexander G. McAdie, quoted in the *Monthly Weather Review*, September, 1904, page 420, says:

The accidents have a scientific interest in that there are but few records of deaths by lightning in this state. But it should be noted that comparatively few people have been exposed to storms at high elevations. Mr. Byrd Surby was killed on the summit of Mount Whitney, within 50 feet of the monument. It was snowing at the time of the accident. It is probably not well known that the variations in the electric potential of the air during a snowstorm are almost as rapid and as great as those prevailing during a thunderstorm. In this present case I am inclined to think that the electrical disturbance was not localized, but simply incidental to a disturbed field which extended well over the high Sierra, Inyo, Panamint and Telescope ranges; also the San Bernardino Range, and probably the mountains of Arizona. This condition lasted perhaps a fortnight.

Then the electric display began. First a dull detonation to the south, and, after an interval, a flash at the observatory window as if there were wires in the observatory and electricity had struck them. To this we paid little heed, for the occurrence was trivial. After a time, however, a crash a hundred feet below us and perhaps 500 feet away and the immediate terror of the horse drew us to the door.

As we emerged, every artificial projection on the summit was giving forth a brush discharge of electricity. The corners of the eaves of the observatory (covered with Malthoid roofing), the arrow of the wind vane, the cups of the anemometer, each sent forth its jet, while the high intake pipe of the precipitation tank on the apex of the summit was outlined with dull electric fire.

Whenever our hands arose in the air, every finger sent forth a vigorous flame, while an apple, partially eaten, in the hand of Captain Brambila sent forth two jets where the bite left crescent points. This latter phenomenon occurred, however, only when the apple was raised above the head and ceased when it was lowered so that the eating of the apple involved no visible eating of flame.

To cap the climax my felt hat above the brim flashed suddenly into flame. I could feel the draft and, it seemed to me I could hear it too. The halo was dazzling, but before the senses could react it was gone. I had earlier but ineffectually rubbed Captain Brambila's hair to elicit a discharge of electricity, but because he was diminutive in size, nature had selected me to serve as the point of electric discharge. So vivid were the flames that continued to play from the corner of the observatory that I reached up to assure myself that the observatory was not actually on fire.

We felt no ill physical effects nor any special alarm, but for prudence's sake we sought the interior of the observatory, where the pranks of the electricity were apparently completely avoided. About 7:30 o'clock, an hour after the electric storm had burst, it had vanished.

The clouds, however, continued to hover around the summit, and the following evening a heavy rainstorm swept from the mountain earthward toward Reno, gaining violence as it descended, until the valley was drenched. We followed the storm closely with but little inconvenience from rain.

Only once before have I met electricity actively present on Mount Rose. This was during daylight of June 25, 1906, in a wet snowstorm accompanied by dense fog. At that time the thunder was pealing in the abyss below me, until I felt like some Jupiter hurling thunderbolts at the earth beneath.

The puzzle is that the tension did not burst on the apex of the summit where the discharge of earth electricity had particularly ionized the air, instead of upon the rocks below. A possible reason may be found in the suggestion of Dr. R. S. Minor that the scud which was sweeping between the heavier clouds above and the mountain mass may have become electrified by passing between the two poles and then have discharged its electricity as it was swept down nearer the mountain where the air currents swirl in its lee.

So far the discharges on Mount Rose have occurred at this lower point, and this habit may prove to be the observatory's security. The large extent of the summit over which the brush discharge was active, and the intensity of the latter, indicate imminent danger to the entire observatory. It was believed, when the observatory was planned, that the bolt would be induced to strike the high intake pipe of the precipitation tank on the crest; but such a conductor, it seems, would prove insignificant on account of the gigantic proportions of the electric activity. Besides, it is impossible to create a satisfactory circuit from tank to mountain, for the summit is apparently one mass of shivered rocks whose interstices are filled only with dry earth.

A wire cage in which to sit during thunderstorms has been suggested as affording possible immunity for the observers. It is possible that the observatory itself, which is sheathed with Malthoid roofing above and nested in the rocks below, may serve the same

purpose. The placing of wire netting around the louvered shelter where the meteorograph is installed might give this instrument protection from electric shock, but the projection of the wind vane and anemometer masts from the shelter may attract sufficient electricity to fuse the netting and reach the instrument by way of the mechanical connections. There has been actual danger on Mount Rose, so far as known, only this single time during the past three years.

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THE BLOWING OF SOILS

WHEN a boy on my father's farm in Iowa I experienced a three days' dust storm, when the air was so full of dust that one could hardly see, and the sun was almost obscured. After the storm I noticed that drifts of loose earth had accumulated in all the protected hollows, in the lee of corn shocks and fences, and on the grass lands adjacent to the cultivated fields. We had no other storm so severe as this one, but I noticed that the drifting soil particles in the air were gradually filling up a small pond that adjoined a plowed field. This little pond was so situated that no wash from the plowed area could enter it, and its filling up was undoubtedly due to the deposition of wind-blow particles. Some years ago I again visited the place and found that this pond was dry and filled up practically level with the surrounding land.

In the spring of 1889 I had occasion to observe the erosion by the wind of a recently plowed field on my father's farm, near Yates Center, Kan. The field sloped gently toward the southwest, and for several days the wind blew violently from that direction, carrying away the soil in some of the most exposed places to the full depth to which it had been loosened by plowing. Most of the soil was, of course, dropped in the lee of the first obstruction; but the finer particles were probably carried for many miles. Other fields in the vicinity suffered the same fate, and after the storm was over our neighbor adjoining us on the north had the bulk of our soil. Most of it was piled in big drifts just back of an osage hedge.

Later, near Albuquerque, N. M., and at Cibicu, Ariz., I was impressed by the strong indications of eolian origin offered by the adobe clay of this region. This material can hardly have been formed by glacial action as no traces of such can be found, and fluvial action is in most cases excluded by the topography. The deposit is found superimposed on many geologic formations and at a wide range of altitudes. It may be partly rain wash from the hills, but were it entirely so it would show a non-uniformity of mechanical composition—becoming finer in grain with distance from the hills. This does not occur—the deposit being very uniform. It is also remarkably level, showing no traces of the descending slopes characteristic of alluvial fans. Becoming convinced that this deposit was largely of eolian origin, I began to look about for evidences that deposits are made and materials moved by the wind. The following observations resulted:

1. On the Jemez coal lands near Albuquerque, N. M., it was noticed that, on windy days, a sheet of dust was continually blown completely over the region from Mesa Blanca and adjacent ridges.

2. At Cibicu, Ariz., fine dust settled on anything spread on the ground after a reddish southwestern sky at sunset, indicative of dust storms in the Gila-Salt River desert many miles away. This happened even when there was apparently no wind at Cibicu during the previous night and the day.

3. During a year of very slight rainfall at Fort Apache, Ariz., the adobe flats received even more increment than in years when the rainfall was normal. In this region the soil particles are continually being blown to the eastward from the region to the south of the long tongue-like ridge of the Mogollon range which extends to within a few miles of Fort Apache, and the dirt is being collected in the grass-covered area to the leeward of this ridge, but so slowly that the growth of vegetation keeps pace with the deposition.

4. The accumulation of wind-blown earth is

so great in the lower Salt River and San Y Sidro-Zia regions, New Mexico, that the cactus and yucca plants are half submerged by it. In the vicinity of the Indian village of Santa Anna, lower down in the same valley, the wind-blown sand has in recent years completely covered the farm lands of the tribe, so that the government was finally compelled to give them another reservation across the Rio Grande at Bernalillo.

5. At the Indian village of San Felipe in the valley of the Rio Grande, the settling of dust particles blown from the almost barren mesa adjacent is very noticeable.

6. Around the White Thunder camp of the Rosebud Indian Reservation, in the valley of White Thunder Creek, S. D., the soil is a clay; yet the sands from the Arikaree strata some six miles away are blown by every wind storm completely across the valley and even into the houses, so that after every heavy wind one can write in the sand on the window sills inside the houses.

7. Last year I laid down a board in some grass on the lee side of Pacific street ridge in the village of LaPush, Wash., near the beach of Quillayute Bay. Several months later I looked at the board and found it covered with one eighth of an inch of beach sand which had been blown about half a mile and over the above ridge, which is completely covered with grass.

Various means have been adopted to prevent the movement of soil by winds. The Moqui Indians do not plow their soil at all. They simply dig a hole in the sand for each hill of corn and then tramp down the dirt with their feet to keep it from blowing away. Many people in the southwest do not plow their ground until the windy season is over. And in the irrigated regions the ground is flooded as soon as plowed. To prevent the movement of soil by the wind as well as to level the land, the farmers of the plains region roll their land or crush it with a weighted plank float. So far as the writer knows, these are the only means now used by farmers to keep the soil from being blown away by the winds. But others could be employed. Groves and hedges could be planted on the windward side of fields to break the winds. Also, at least for small farms, wind breaks like those used by railroads as protection against snow and sand drifting in cuts could be used to advantage, especially in regions too dry for the rapid growth of trees and hedges.

Albert B. Reagan

LaPush, Wash.

A FAULT IN AN ESKER

About three quarters of a mile from East Templeton, Mass., on the southeast side of the direct road from that town to Gardner, there is a cutting in one of the large, esker-like ridges typical of this locality. The deposit consists of distinct layers of fine, compact sand, with a few beds of gravel, of which the pebbles vary in diameter from less than an inch to six inches.

Where the stratification is well marked, near the northeastern end of the pit and about half way up the slope, the horizontal beds are found to terminate abruptly against a flat, narrow layer of sand and gravel, striking roughly east and west and dipping 63° northwards. This layer can be clearly discerned for a distance of more than twenty feet on the face of the pit; but above and below, like the beds which it traverses, it is covered by loose slide material.

That this layer may represent a fault zone, analogous to a fault breccia, in which the slipping destroyed original structures, is suggested by two facts: First, the beds are displaced; where they are best shown, the order of coarse gravel (2 ft.), fine, cross-bedded sand (2 ft.), fine gravel (4 in.), and very fine, compact sand (10 in.), on the south, is repeated, on the north side of the fault, about two feet lower. Second, the strata on the south side are plainly bent downwards next the fault.

Whether this dislocation is restricted to the glacial deposit only, or extends down into bed rock, can not be determined, for no bed rock outcrops here. The first supposition (of limitation to the deposit) seems most reasonable, since (1) the plane of the displacement is near the steepest slope of the deposit; (2) it strikes more or less parallel with the length of the

deposit; and (3) post-glacial faults are generally of only a few inches displacement. Possibly the slipping was due to the removal of sand by water running below the surface. Certainly all the evidence militates against the assumption that the digging of the pit could have been the cause.

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BUFO FOWLERI (PUTNAM) IN NORTHERN GEORGIA

IN the September issue of SCIENCE for 1907, I discussed the range of Fowler's toad to some extent, according to my own observations. An opportunity to spend the spring and summer of 1908 in the counties of Gwinnett and Jackson in northern Georgia has enabled me to make further observations concerning this interesting and apparently little understood toad.

In this region throughout March I heard the occasional, prolonged trills of the so-considered common toad (*Bufo lentiginosus*). By the first of April these notes had become quite silenced, and the distinct chorus of congregations of Fowler's toads had begun. I first noticed these toads singing on the evening of March 26, although I think the first singers had appeared somewhat earlier. It was interesting to note that the voices of Fowler's toads were never heard with the appearance of cool, chilly nights, although the trilling of the common toad continued. Throughout the early spring, this contrast in the occurrence of the two notes, with respect to temperature changes, was very marked.

Fowler's toad in this region of Georgia is an exceedingly abundant species. Throughout the months of April, May and June its droning cries are heard in thousands along certain streams. At this season the females are laying their long, bead-like strings of eggs in the water, attended by hosts of noisy males. Especially during the spawning season, the females seem to be greatly outnumbered by the males.

During the last week of May, the streams and pools where the eggs had been laid, were alive with tadpoles in different stages of development. About the middle of June, many of these had developed into tiny toads which were hopping along the banks, and in a few days every tadpole had disappeared as if by magic. A few evenings later, there was a noticeable increase in the number of males in voice along this stream. On visiting the locality, I was interested to find the females again laying eggs in great quantities, accompanied by many males. It would seem from this that these toads may have several well-defined egg-laying periods in a season.

After the spawning season these toads leave the water and take up quarters in the fields and pastures. During the day they generally remain quiet beneath stones, logs and bunches of grass. I find them very frequently in deep gullies. Here also I have found their eggs in the transient pools following showers. Several times I have found these toads buried to the eyes in sand greatly heated by an intense sun. In gullies and banks by the roadsides, the horizontal holes left by the decay of tree-roots, are favorite hiding places for these toads during the day. Several sometimes occupy the same tunnel, and may be seen peering out with expressions evincing serenity and contentment. Fowler's toads are rather inclined to be social in their habits. Last summer, near Hartford, Conn., I noticed a great stone door-step under which fifteen or twenty of these toads had taken up summer quarters. Every evening throughout the summer they would appear, one by one, and hop in a long line, up the walk leading into the fields.

I find considerable variation in the size, markings and coloration of Fowler's toad. The general coloration varies from a bright reddish brown to a dark grayish brown. Beneath, I have found no markings whatever, in either sex.[1] In truth, in this region of Georgia every toad examined was, for this and other reasons, apparently a Fowler's toad.

The usual note of Fowler's toad is a brief, penetrating, droning scream. Only once have I heard a decided departure from this. I heard this note late in April, in Gwinnett Co.,

[1] A single small, dark spot in the center of the breast of the males is the only marking I have ever observed beneath.

in upper Georgia. A single individual of a noisy congregation of males had the unmistakable trill of the common toad, but short and decisive like the Fowler's song. It was a perfect combination of the notes of both.

Wherever I have found this toad—in central and southern New England, around Washington, D. C., and in northern Georgia—it has been the only common form. Throughout the region of Jackson and Gwinnett counties of northern Georgia this toad is extremely common. Whether or not its range extends into the central or southern portions of the state, I have not determined. It is evident that *Bufo fowleri* occurs abundantly in much of the territory east of the Appalachian Mountains, and is far from being an uncommon or local race or species.

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A PRELIMINARY NOTE ON A GROUP OF LACTIC ACID BACTERIA NOT PREVIOUSLY DESCRIBED IN AMERICA

Various bacteria forming acid in milk have been described. The organisms most frequently met and which are of the greatest economic importance are those belonging to the group represented by *Streptococcus lacticus* (Kruse) or the *Bact. lactis acidi* of Leichmann. This group is characterized by the small amount of acid which the organisms are able to produce in milk. The milk acted upon by pure cultures rarely shows an acidity exceeding one per cent. The limiting factor is apparently the formation of free lactic acid, the organisms being unable to grow in the presence of free acid. As numerous investigations have shown, the amount of acid produced varies with the composition of the milk. Milks high in casein and ash constituents develop a greater amount of acid under similar conditions than do milks whose casein and ash content is lower, because these substances combine with the acid formed.

Freudenreich, in Switzerland, has described a class of lactic-acid-forming bacteria which are able to produce much greater amounts of acid in milk than the organisms of the *Streptococcus lacticus* group. This group of high acid-forming organisms has been brought into prominence recently by the work of Metchnikoff and others on the fermented drink yougurt which is prepared from milk. The organisms found in this type of fermented milk are characterized by the production of large amounts of acid, three per cent. and over, by the high optimum temperature for growth, 42–45° C., by growing only under certain narrow conditions on artificial media, and by their morphology, being large bacilli. In all these the organisms from yougurt agree with those described by Freudenreich.

It has not been thought that such organisms are widely distributed. Indeed some writers have asserted that this type was peculiar to the country to which yougurt is native, Bulgaria. Within the last few months it has been found that organisms whose characteristics are similar to those found by Freudenreich in Swiss cheese and to those found in yougurt are of common occurrence in this country.

If a sample of mixed milk is placed in a tightly stoppered bottle and incubated at 37° C. the acidity rapidly reaches one per cent., due to the growth of *Streptococcus lacticus*. The acidity then continues to increase slowly until at the end of two to three weeks it reaches 2.5 to 3 per cent. The flora at first made up almost wholly of the small diplo-bacilli changes, through the appearance of long slender bacilli, which increase in number with increasing acidity.

In cultural characteristics and in biochemical reactions, the organism isolated is apparently of the same group as those of Freudenreich and the bacteria in yougurt.

As far as the writer is aware, this type has not previously been found in this country, although it is of wide distribution, and has been present for years. Milk bottled in 1902 was recently opened, and showed an acidity of over 3 per cent. A detailed study of the distribution and characteristics of the organism is being made.

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